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STUDIES IN PSYCHOMETRIC THEORY[1]

BY ELMER CULLER

University of Illinois

XI. The Effect of Practice on the Distribution of Judgments

Do ordinary differential judgments (warmer, lighter) approximate more and more nearly, with increase of practice, the normal percentile curve? When a comparison-weight rises step by step in relation to a given standard, will the resultant series of 'heavier' judgments, rising also step by step between 0 per cent. and 100 per cent., tend to approach more and more nearly, as practice accumulates, the form of the cumulative $\Phi(\gamma)$-function? This is our problem; and we hope to show herein that (1) when degree of normality (P) is high to begin with, it does not appreciably change as practice proceeds; (2) when P on the contrary begins low, it improves notably as practice continues.

The matter has gained some attention of late (rightly, in our view) from students of psychometric theory; it reaches almost every phase of quantitative psychology and thus goes far beyond the confines of psychophysics and differential sensitivity; so that we incline to Urban's declaration that if practice does indeed prove to bring an observer's judging into better accord with the $\Phi(\gamma)$-hypothesis, the fact will have "far-reaching practical and theoretical consequences" (*12*, 496). Despite the reach and import of the question, it has so

[1] The first ten of these *Studies* have already appeared in *Psychological Studies from the University of Illinois* (*Psychol. Monog.*, 1926, 35, no. 163), 56–137.

far been treated in a rather casual way; it has not been seriously and systematically taken in hand; so that we have had a good deal of debate with little in the way of evidence or assured conclusion. We here propose therefore to submit a few facts and inferences in the endeavor first, properly to analyze and define the problem and then to attempt an answer. While the evidence is not broad enough to provide a universal solution, it does reveal, as we hope to prove, some clear and concordant tendencies. The present argument falls under two heads:

(1) In the case of a $\Phi(\gamma)$-function, what is the true measure of agreement or accord between theory and observation?

(2) Does the accord improve with practice (successive samplings from a temporal sequence)?

As for (1), there is no accepted procedure to date for finding how well theory conforms to observation, no consensus among investigators on the true way to measure how widely Urban's curves depart from the empirical values to which they are fitted. One method has been developed by Thomson (*9*) and used by Rich (*8*); another (the traditional procedure) was originally adopted by Urban (*10*), later used by Hoisington (*7*) and Fernberger (*6*) and accepted by Boring (*1*). The two procedures, of course, differ; but the superiority of Thomson's can readily be shown. By the 'best-fitting' curve of a given kind we mean the most probable curve of this type or formula that can be laid through the body of data in question; that curve in turn is 'most probable,' by the accepted theory of least squares, whose Σd^2 is smaller than for any other function of the same type that can be used to graduate these data. Now the distinctive feature of the Urban-hypothesis, which alone differentiates it from Müller's psychometric formula, is that Urban multiplies each d^2 with $1/pq$. When $p = q = 1/2$ this factor amounts to 4.0, but when $p = .01$ or .99 the reciprocal of pq rises to 101.0, or more than 25 times as large. With Müller, then, one d counts just as much as another, all have the same importance; every frequency contributes equally to the course of the fitted curve; whereas Urban penalizes a d near 0 per cent. and 100 per cent. as being far

more serious than in the region of 50 per cent.; these extreme values influence the course of the smooth curve some 10 to 25 times as much as do the medial percentages. It follows that Urban's $\Phi(\gamma)$ fits the tails far more neatly than the middle range of its distribution, a procedure which is both demanded and warranted by the greater stability of judgment near the extremes. Since all these curves are so fitted as to minimize $\Sigma(d^2/pq)$, this sum is obviously the only true index or measure of agreement between theory and observation; of n empirical distributions, the one whose graduated curve shows the lowest $\Sigma(d^2/pq)$ is by definition the 'best fitting' of the lot. Inasmuch as the raw Σd^2 has no stated relation of any kind to this weighted sum, which alone is used in graduating the observed figures, it would be strange indeed to make use of the former when comparing goodness of fit. Σd^2 is commonly used to measure goodness of fit merely because functions are commonly so fitted as to minimize Σd^2; but when Σd^2 has no part whatsoever in fixing the course of the fitted curve, it has likewise no place in measuring how well the resultant curve fits. Inasmuch as Urban has never defended the adoption of Σd^2, we need stay with the matter no longer.

(2) Does goodness of fit, when properly measured, improve with practice (samplings taken from a temporal sequence)?

Here as in question (1) opinion is conflicting and tentative. Thomson (3, 90) discovered that, in the case of Urban's seven observers, the best approximation to $\Phi(\gamma)$ appeared with the second (II, Urban himself), who was presumably more practiced than any of the others. Hoisington also finds that fit improves with practice: "we approach the theoretical function as we increase the number of observations. This relationship suggests that the $\Phi(\gamma)$-hypothesis is the correct hypothesis for these conditions, and that deviations from it are due in actual cases not to errors of theory but to errors of observation" (7, 595). Urban, being impressed with these two findings and believing that confirmation of them would have far-reaching practical and theoretical consequences, asks other investigators to put the same question to any data they

may have (*12*, 496); whereupon Fernberger computes Σd^2 for some earlier figures of his own (28 sets of 50 each for two observers) and concludes that, while the decrease in Σd^2 is far from regular, the general tendency toward improvement of fit is evident (*6*, 500). Rich testing the same data by Thomson's method finds that, "No general conclusion as to the effect of practice seems possible from the data at hand" (*8*, 620). Boring finally is impatient of the whole problem and contends that seeking for *any* general psychometric function [*e.g.*, $\Phi(\gamma)$] is like "following a wandering fire," the likelihood of finding a generalized formula being so meager as to make the topic unsuitable even for investigation (*1*, 770). Despite these conflicting and hesitant conclusions, we believe that the problem, when better analyzed and treated with additional evidence, will put us in the way of its own solution.

The value of *P* (Pearson's symbol for goodness of fit, which may range from .00 to 1.00) is of course fixed by the concurrence of many factors (*e.g.*, observer, type of material, categories of response); but the two which alone concern us here are: (*a*) how *P* varies with practice (the temporal position of a given sample); (*b*) how *P* correlates with *h* (the measure of precision, whose magnitude varies inversely to the limen). Let us consider the two cases in turn. (*a*) If we are to speak usefully about the matter, we need to distinguish two forms of 'practice'; first, general facility in the technique of observation, and secondly, specialized skill in some particular function (as, discriminating weights, greys or temperatures). A 'practiced' observer in the general meaning is conversant with the methodology of scientific observing, knows by dint of adequate training how to form and use criteria, is careful to keep conditions within and without the organism as constant as may be, and the like; he is then a trained O, though he may never have lifted a weight nor compared two thermal impressions. When he begins to observe in a particular field (lifted weights), is there any reason to suppose that his responses will of necessity distribute more and more normally as time passes? Not at all; if he uses the same care in judging at first as he does later, his distributions will not appreciably

change in type. In psychometry, so far as known, normality demands nothing but a situation in stable equilibrium; it has no necessary dependence upon temporal order; a beginner who is careful about criteria and attitude may accord just as well with normality in the first stages of his work as in the last, even though his sensitivity or limen shows meanwhile great improvement. If on the contrary O at first is naïve, ignorant of good methodology and indifferent to the demands of his work, then indeed will his judgments be erratic, because they issue from instable and ill-defined criteria; his frequencies may then be expected to depart widely from their theoretical values. As he proceeds, he will tend not only to lower his limen but to define and stabilize the criteria as well; but the reader will note that these two forms of 'practice,' though they may of course proceed concomitantly, are by no means identical nor do they always correlate. This being true, the question whether practice leads to greater accord with normality can be answered only when we know what kind of 'practice' is meant; debate is irrelevant until the situation is properly defined. In short, an observer whose criteria and attitude improve pari passu with his increase in proficiency will distribute his frequencies more and more normally as judging proceeds; while an O who is careful and consistent from the start will show little if any change, even though his precision be markedly increasing all the while.

We now come to question (*b*): How does *P* correlate with the precision, *h*? Now *h* (the traditional measure of precision) is, in our opinion, the true index of differential sensitivity by the Urban procedure (see article IX of this series: 5). If we define the limen as the probable error of the $\Phi(\gamma)$-curve which is fitted to a given set of frequencies, then *h* varies inversely to the limen by the simple relation

$$h = \frac{\cdot 4769}{\text{p.e.}}.$$

h may range from zero to infinity; when *h* is zero the limen (p.e.) is infinite and when *h* becomes infinite the limen is zero. Graphically, this means (Fig. 1) that the ogive begins with a

straight line rising vertically from 0 per cent. to 100 per cent. ($h = \infty$) in which case the limen (p.e.) is clearly zero; an infinitesimal increment of stimulation (weight) is attended by a change from 100 per cent. lighter to 100 per cent. heavier

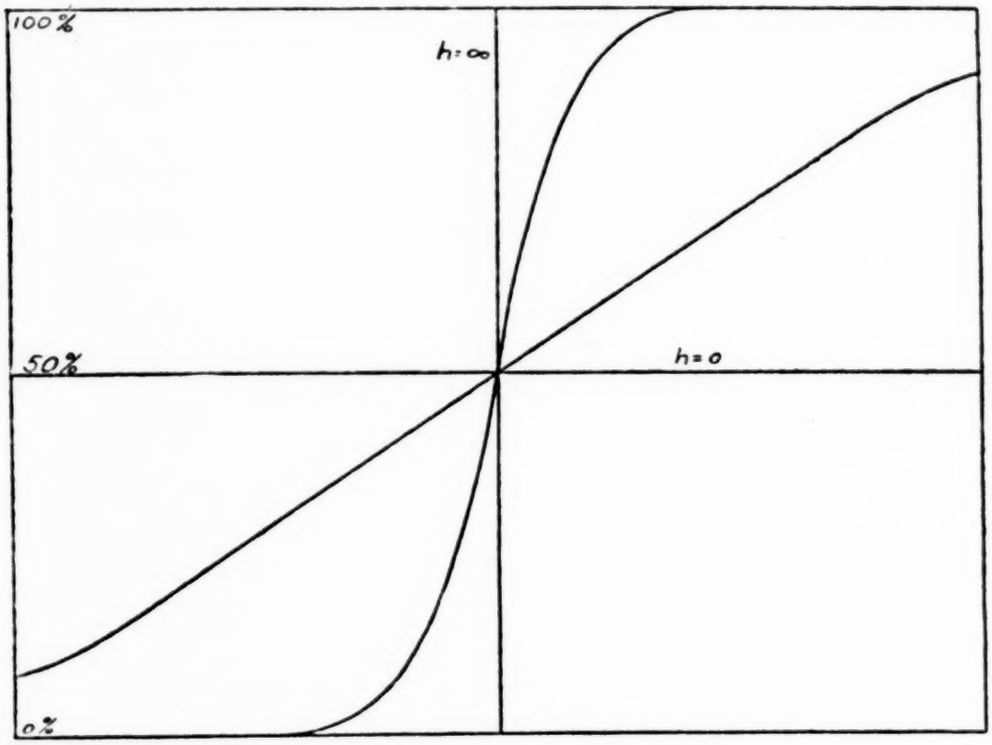

FIG. 1

judgments. From this extreme the curve passes through a series of stages to the other limit, a straight horizontal line, where $h = 0$ and the limen is infinite; in this case, no increment of stimulation, however large, has the least effect upon the proportion of heavy and light responses.

Now it is clear that P and h will not correlate perfectly. A steep curve (high h) is not ipso facto more normal than one of gradual slope. Obs. A may be far more sensitive to change of stimulation than is B; and yet B may be just as careful and consistent in his judging as A and therefore distribute his responses just as normally. B may have less refined criteria (lower h) than does A, and still may use them with equal care and control; thereby distributing his frequencies with equal normality.

There is, however, an important 'one-way' relation be-

tween fit (P) and precision (h). When h is high, P must also be high; when h is low, P may be either low or high. A good fit (high P) is a necessary but not a sufficient condition for good precision (high h). Normality is possible without high precision, since a curve with low h may be perfectly normal; but high precision is impossible without normality. The truth of these propositions can readily be shown. If the reader will plot a set of curves from low to high precision (Fig. 1), he will see that as the curves steepen, they approach the limiting asymptotes (0 per cent. and 100 per cent.) with great rapidity; they fairly hug those extreme lines up to the point of inflexion, whereupon they move abruptly away. Such 'high contact' is possible only when the observed and the graduated frequencies are almost identical. Consider an urn with 98 white and 2 black balls, white standing for 'heavier' and black for 'lighter.' From 100 comparisons (draws) we should then expect 98 heavier (white); but we may of course actually get any number from 0 to 100. The proportion of heavier (p_H) may then conceivably fall short of the 'true' theoretical value (.98) by as much as .98 but can never exceed it by more than .02. What follows from this lopsided variability—a possible range of .02 above and .98 below? Simply that the O, when careless and inconsistent, is far more likely to fall *below* the theoretical value than above it; that is, if the successive frequencies (p_{H_1}, p_{H_2}, . . .) to which the curve is fitted are erratic, especially in the tails which count so heavily in Urban's procedure, the curve is much more likely to be pulled *away* from the limiting asymptotes than toward them; with the result that the slope or precision (h) is bound to be reduced. This effect would vanish if the possible range for p_H above and below .98 were equal; only because a negative deviation is so much more easy and likely than a positive do we have the present result. Around 50 per cent. variations up and down are equally probable; at all other points of the distribution, however, the deviations are unbalanced and tend to favor one side. This means that a distribution of erratic frequencies, now too large and now too small, will *inevitably* flatten the curve, making the percentages above 50 too low

and those below 50 too high. An erratic O is thus bound to have not only poor fit (low P) but low precision (h) as well; whereas high precision can be attained only when P is also fairly high. By the $\Phi(\gamma)$-prodecure it is impossible for an observer to achieve the highest precision (h) unless he distributes his judgments normally. Any deviations from the theoretical (true) value are more likely to lower h than to raise it; maximal precision can be realized only when the observed and adjusted frequencies absolutely coincide. As h becomes infinite, P must be perfect (1.00); but when h is zero, P may range from .00 to 1.00.

Let us now revert to the question: Do P and h tend to correlate? Suppose we drew from an urn (α) successive distributions, testing each for goodness of fit (P); and with each draw from α suppose we drew from a second urn (β) another distribution, which we tested for precision (h); setting each value of P beside the corresponding value of h. No correlation would in general exist between pairs of P and h thus drawn at random; a high P might go with high, low or medial h, so also a low P; there would be no reason in the experimental situation for any trend to appear. If we now proscribe however all the pairs (low P – high h), a trend will at once appear; high P will now tend to correlate with high h, even though the agreement will be far from perfect, since high P may still go with low h. This is just what the Urban method does to a set of distributions; the combination, low P – high h, becomes less and less likely as h increases; some correlation may thus appear between h and P. Good fit (high P) will not of itself increase precision; but a poor fit may well operate to reduce the magnitude of h.

We conclude then that when P is high from the start, there is likely to be no correlation at all of P with h or of P with temporal position; in other words, neither increase of precision nor continued practice will have any effect at all upon goodness of fit; when observer starts with low P, however, an increase of precision is almost certain to bring some improvement in fit as well.[2]

[2] What is a 'low' P and 'high' h? In a later article of the present series (XIII), a table is proposed showing the range of good, fair, poor P according to the characteriza-

Let us now look at the experimental evidence and see how it accords with the analysis above presented. First, we have some data from the writer's study on thermal discrimination (*4*). The frequencies for every time-space order at all the temperatures covered in our principal method (*A*) were tested for precision (*h*) and for normality (*P*); just 92 cases in all, 46 of warm and 46 of cold. The labor of curve-fitting made it desirable to limit the process to a reasonable number; and inasmuch as these 92 cases comprise every distribution by our principal method, without exception, we may assume that what holds here is typical of all our data.

TABLE I

		32°	28	24	20	16	20	24	44	40	36	32	28°
W	*S–J (L)*	.60	.81	.88	.79	.74	.00	.98	.01	.06	.03	.00	.93
	J–S (L)		.99	.91	.00	.25	.00	.23	.06	.96	.68	.93	.73
	S–J (R)	.70	.97	.51	.99	.85	.00	.44	.76	.97	.95	.13	.01
	J–S (R)		.82	.72	.38	.56	.30	.98	.00	.56	.91	.17	.95
C	*S–J (L)*	.03	.34	.41	.19	.91	.01	.98	.41	.05	.21	.48	.97
	J–S (L)		.78	.88	.20	.92	.10	.32	.01	.47	.72	.63	.31
	S–J (R)	.91	.93	.39	.85	.91	.00	.32	.41	.81	.64	.84	.99
	J–S (R)		.98	.82	.00	.76	.01	.07	.14	.17	.26	.96	.45

Values of *P* (goodness of fit) for 92 distributions from the writer's study on thermal discrimination (*4*). The temperatures are given in the order with which they were covered. *W* denotes warmer and *C* cooler; *L* means that comparison-jar was to the left of standard, *R* to the right; *S–J* that comparison-stimulus *J* followed standard in time, *J–S* that *J* preceded *S*. Since the figures for *h* are given (*4*, Appendix), they are not repeated here.

Secondly, we have data from Obs. I, II, III in Urban's study (*11*, 276). There are nine series of 50 trials or 450 judgments for each pair of stimuli; the latter range from 84 to 108 grams by 4-gram steps, the standard being 100. These three are assumed to be typical of Urban's group of seven observers, and are better for our purposes than the remaining four who covered but six series instead of nine. The values of *P* for the nine series are here given; the correlative values of *h* may be found in (*11*, p. 276), and need not be repeated here.

tion of known authorities. The magnitude of *h* depends on the mode of stimulation (weights, temperatures) and the unit of measure (gm, oz). With stated conditions, a skilled mathematical analyst should be able to provide some kind of general solution; we can do no more than indicate the principle which operates here.

TABLE II

	1	2	3	4	5	6	7	8	9
Vp. I—Lighter	.49	.19	.63	.14	.05	.17	.69	.83	.09
Heavier	.06	.13	.00	.36	.00	.12	.52	.65	.00
Vp. II—Lighter	.20	.99	.76	.30	.98	.20	.44	.44	.83
Heavier	.75	.40	.40	.70	.81	.12	.12	.38	.95
Vp. III—Lighter	.00	.72	.94	.01	.96	.10	.66	.93	.97
Heavier	.11	.57	.95	.09	.32	.02	.34	.71	.97

Thirdly, we have the figures for two observers, II and VIII, cited by Urban (*11*, 294–298) from Fernberger's study: 28 series of 50 trials for each of the comparison-weights, of which there are six (84, 88, 92, 96, 104, 108 grams), the standard being 100. This body of material is quite extensive and has occupied no less than three writers, Fernberger (*6*), Boring (*1*), Rich (*8*); but their discussion has led so far to no concordance. The values of P have been computed and reported by Rich (*7*, 617) and will not be repeated here; the correlative values of h are given by Urban (*11*, 297; Tabellen 18 and 19).

Finally, we extract some data from Brown's extensive study of lifted weights (*2*). On pages 14–15 he gives the number of 'heavier' in 46 series of 50 trials with each of 19 comparison-weights, 43,700 reports in all. To graduate and test all of these 46 distributions would have been prohibitively burdensome; and inasmuch as any reliable tendencies which may be present are likely to appear with equal clarity in a smaller number of cases, we used only those distributions (with standard 100 gm) which are removed by at least three intervening series from any others with which they are to be compared. To abridge the labor of curve-fitting, we used only the frequencies for the odd-valued stimulus-differences (1, 3, 5, 7, 9, 11, 13, 15 gm); after h and the mean had been found, each curve was tested for fit (P). The values for h being nowhere else reported for these distributions, we here include them with the figures for P.

TABLE III

Series No.	1a	5a	17a	19a	23a	1b	5b	17b	19b	23b
P	.61	.89	.57	.99	.89	.73	.85	.99	.44	.70
h	.104	.097	.088	.171	.158	.074	.082	.094	.090	.078
Series No.	12a	16a	18a	20a		12b	16b	18b	20b	
P	.85	.42	.92	.51		.27	.94	.92	.46	
h	.126	.094	.100	.108		.080	.084	.092	.082	

What now do these four sets of experimental facts reveal? Consider first the thermal data from Culler. We compare [3]

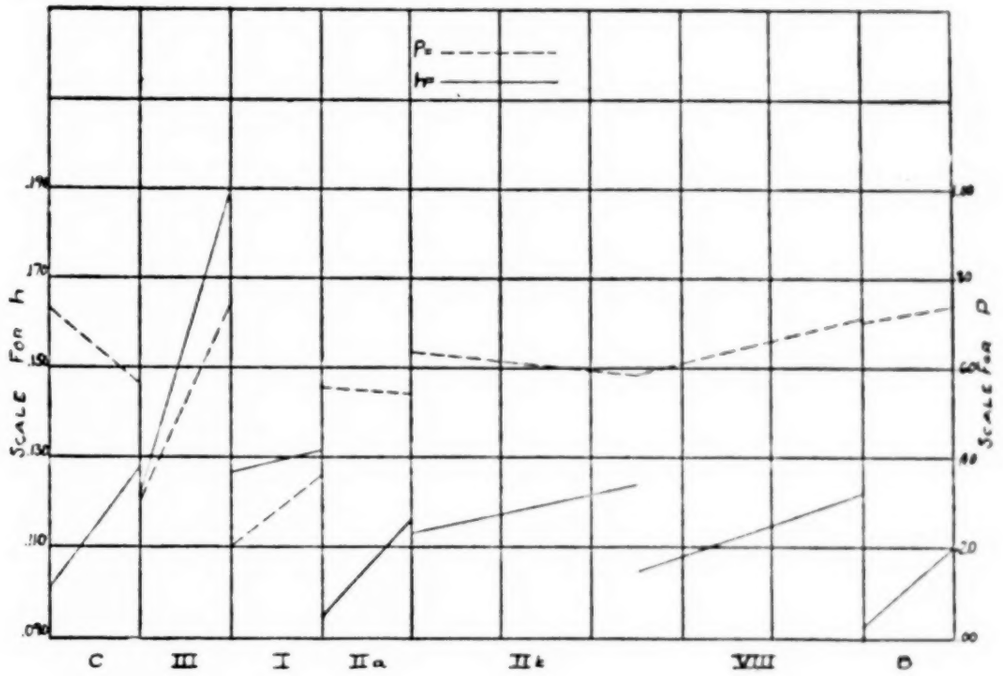

FIG. 2.

the first value of *P* and *h* with the second figure to which it corresponds in every detail (temperature, *S–D*, time- and space-order); thus *P* for *W*, *R*, *S–J*, 32°, is .70 in the first and .13 in the second series — a decrease of .57. Of these pairs we find ten for Warmer (2 at 32°, 4 at 28°, 4 at 24°) [4] and ten

[3] The method used here is explained in the article following (no. XII), to which the reader is referred for details of interpretation and procedure.

[4] The figures at 20° are excluded for the reason that practice-improvement, as measured by the limen or by *h*, had ceased at this stage of our work (thus, from first to second series at 20 , *h* increases four times and decreases four times); by including these figures therefore we should merely obscure the tendencies where practice is still operative.

for Cooler; that is, ten changes (+ or −) for Warmer and ten for Cooler. Now if *P* improves with practice, the fact will appear in these comparisons, there will needs be a preponderance of increase; whereas if the up and down changes are about equal in magnitude, it follows that practice had no appreciable effect upon goodness of fit. Here are the figures:

TABLE IV

	Warmer		Cooler		Combined		Validity exceeds [5]
	+	−	+	−	+	−	
P amount	.51	3.60	1.79	2.38	2.30	5.98	
per cent.	.124	.876	.429	.571	.277	.723	.800
h amount	22.38	4.22	20.04	2.40	42.42	6.62	
per cent.	.841	.159	.893	.107	.865	.135	.957

By summing the first member of all the pairs and likewise the second, and taking the mean, we can see the trend from early to later stages of practice. The figures:

TABLE V

	P		*h*	
	Initial	Final	Initial	Final
Warmer	.79	.54	7.00	8.82
Cooler	.65	.57	7.16	8.93
Mean	.72	.56	7.08	8.87

The mean values [6] are graphically shown in Fig. 2, *C*.

From these tabulations two facts emerge:

(*a*) The index of normality, *P*, reveals no tendency whatever to improve with practice; if anything, it gradually falls

[5] In a series of numbers written at random we should expect the proportion of increase and of decrease, as above measured, to be about equal—50 per cent. each. In the case of *h*, here, the increases equal in sum 86.5 per cent. of the whole amount of change. How valid is this departure from 50 per cent.? According to the T-method (Article XII following) the validity of this excess (36.5 per cent.) exceeds .957.

[6] The two values of *h* (7.08 and 8.87) are divided by 70 for inclusion in Fig. 2; they cannot be directly compared with the several values of *h* in lifted weights.

in this case, though the recession is by no means large enough to be valid. Let us note that from five to fifteen weeks of intensive observing (17 to 24 hours weekly) intervened between the first and second member of every pair; so that practice effects had all conceivable opportunity to appear. Their failure to do so cannot be ascribed, say, to the observer having reached the limit of his capacity; precision (h) was markedly improving all the while; of the changes from first to second h, 86.5 per cent. are positive—a preponderance which may be called 'significant,' its validity being greater than .957 or 2.6 p.e.

(*b*) It follows that P likewise fails to correlate with precision; as P slowly recedes, h rises strongly.

In the case of one experienced observer,[7] then, we find that degree of normality correlates neither with practice (temporal position within a series) nor with precision; this O distributes his frequencies just as normally in the early stages as later, even though discrimination meanwhile was rapidly rising.

We turn to the figures from Observers I, II, III in Urban's study. Here we note the amount of change from each value of P and h to all that succeed it (comparing 1 with 2, 3, . . . , 9, then 2 with 3, 4, . . . , 9, and so on); a total of 35 changes (+ or −) for Heavier and 36 for Lighter with each observer. The *increases* (positive changes) amount in magnitude to the proportions here noted:

TABLE VI

	Lighter		Heavier		Mean		Validity Exceeds	
	P	h	P	h	P	h	P	h
I.....	.54	.60	.71	.48	.63	.54	.800	——
II.....	.51	.78	.47	.78	.49	.78	——	.970
III....	.73	.79	.66	.71	.70	.75	.924	.958

[7] The writer had engaged in a long series of preliminary trials before beginning the method of constant stimuli used above; this training, combined with the fact of observing in his own problem, served to give both the ability and the motivation needed for working at a high level.

For graphical purposes, we find the mean of Heavier and Lighter for each of the nine pairs in the h-series (h_L and h_H) and the P-series (P_L and P_H), and then lay a straight line (by least squares) through every set of 9 means; the initial (first) and final (ninth) figures, when so adjusted, are as follows:

TABLE VII

	P		h	
	Initial	Final	Initial	Final
I	.20	.36	.127	.131
II	.56	.54	.095	.116
III	.30	.74	.121	.190

These figures appear graphically in Fig. 2, under I, II*a*, and III, respectively.

As a means of interpretation in finding the true purport of these figures, we excerpt a few statements by Urban. These three observers were all given a "beträchtliche Anzahl von Übungsversuchen" with the avowed end of bringing each to his "Maximum der Übungsfähigkeit" (*11*, 273). This level was thought to be attained when (*a*) O testified that his 'Urteilsbildung leicht und sicher vor sich gehe' and (*b*) the percentage-frequencies of Heavier and Lighter with a given S–D remained from one series to the next approximately constant (*11*, 273). Inasmuch as all three improved in differential precision (h) during the nine series, Urban himself by 22 per cent. and III by 57 per cent., it is clear that his 'Maximum der Übung' is not to be taken seriously; all were still far indeed from their limit when the series opened. The Obs. are also characterized as follows: II (Urban himself) "hatte psychologische Versuche durch mehrere Jahre hindurch angestellt und hierbei eine entsprechende Übung erworben" (*11*, 282). III on the contrary was "wenig geübt und minder geschickt" and "litt unter dem Stolze, möglichst viele richtige Urteile abzugeben" (*10*, 284); his 'Verhalten' on the whole was 'minder befriedigend' (*11*, 279).[8] Obs. I apparently was

[8] Indeed, the data from III are rejected by Urban as too 'unregelmässig' for analysis and reduction (*11*, 279). The difficulty is clear: his proficiency keeps on sharply rising long after he is presumed in theory (by the criteria) to have found his 'Maximum'

in a condition 'mangelnder Übung' (*11*, 293) and is not included with the three 'verlässlichsten' (p. 282) of Urban's seven Obs.; this seems to place him with III.

From tables and graph we conclude:

(1) The precision of II (Urban) is by far the lowest but his degree of normality (*P*) is consistently high; his data support our theoretical analysis in three ways: (*a*) Fit may be good even though precision be low (when above .50, *P* is high, while *h* at .100 is low in relation to I and III and to Urban's later work in II*b*). (*b*) Fit may remain constant (*P* = .56 to .54) even though precision be markedly improving the while (*h* = .095 to .116; in Table VI the proportion of increase for *h* is .78, whose validity exceeds .970 or 2.8 p.e.). When *P* is high and *h* is low, the two may proceed quite independently of each other. (*c*) An O, if well-trained in general technique, is then found to show high and consistent normality from the start, even though his differential proficiency in a given field (weights) be quite low. This we construe to mean that, while his criteria be less refined than those of another, he knows how to use them well and thus distributes his frequencies normally. (2) III is a splendid example of an O who is unpracticed (wenig geübt) in both general and particular. He seems to have a good deal of natural ability for weight-discrimination; his precision starts with .121, well above II and about on a par with I; but even this level is merely the beginning of an extremely rapid rise. His index of fit (*P*) on the contrary is pretty poor (.30); though a little better than I's (.20), it is far below II's (.56); but this again improves amazingly (.30 to .74). His data support our

(practice-limit). By end of the nine series III had not merely the highest precision (which of itself has little moment) in the group but the highest index of normality (*P*) as well. In face of this we can hardly take Urban's adverse characterization at par; objectively analyzed, his data show III to be an Obs. with good capacity and serious motivation; the steep rise in precision, however, shows that his criteria are as yet neither well-defined nor uniform from day to day; he is not yet working on his true level; he cannot therefore be expected to display the same stability and consistence which we find in a seasoned Obs. like Urban. His data, by the very magnitude of their practice-change, set in relief an important factor in the present problem; the reader is warned therefore not to undervalue their significance merely because of Urban's derogatory characterization.

theoretic conclusions in two ways: (*a*) An ill-trained Obs. may be expected to distribute his frequencies anormally at the start, even though his general proficiency be fairly high. Thus III begins far above Urban in precision and far below him in fit. (*b*) When fit is poor to begin with, a rapid and large improvement in *h* is bound to better *P* as well; that *P* cannot remain consistently low when *h* continues to ascend, is clearly exemplified here. As the index of precision climbs, a low degree of normality becomes, not to be sure impossible (there being no such concept in statistics), but more and more improbable. We should say, for example, that if *h* goes above .170 in lifted weights, *P* can no longer stay down around .30. (3) About Obs. I, his degree of practice and general status, we know little, except that Urban seems to rate him with III; his results are therefore hard to construe. Precision begins fairly high (.127) and stays on this plane throughout; *P* on the contrary opens quite low (.20) and then records a marked amelioration (to .36). The ratio of increase for *P* (Table VI) is .63 ,which is higher than for *h* (.54), though neither is 'significant' (validity of $P > .800$). At least one cardinal result however is here again confirmed: when normality is low in beginning, it tends to improve as practice proceeds.

We come to the third set of data for Obs. II and VIII (II meaning as above Professor Urban), which have already been worked by Fernberger and Rich. There being many more series than hitherto, we simply count the *number* of changes, positive or negative (instead of taking their actual magnitude) from each value of *P* and of *h* to all that follow it (comparing 1 with 2, 3, . . . , 28, then 2 with 3, 4, . . . , 28, and so on): a total of 378 comparisons (+ or −) for Heavier and 378 for Lighter. The figures given below (Table VIII) show the proportion of *increases* among the 378 (the remainder being of course decreases); the validity (V) is recorded as above when it exceeds zero. For comparative purposes are included the Pearson coefficients, *r*, for the same data. Every series has a temporal order extending from 1 to 28; the first value of *P* for Heavier (or Lighter) is paired with 1, the second with 2, and so on; we thus have 28 pairs in the

correlation table for Heavier and for Lighter, and 56 for both together. The validity of each coefficient is also recorded to facilitate collation with the corresponding figures of the *T*-method. The reader will find that both methods (T and *r*) closely agree and yield the same inductions; *T* being a conservative procedure, its validity usually appears lower than for *r* (see Art. XII, following).

TABLE VIII

		Obs. II			Obs. VIII		
		Heavier	Lighter	Both	Heavier	Lighter	Both
h	*T*	.581	.566	.573	.587	.593	.590
	V >	.90	.85	.939	.92	.93	.960
	r	.283	.079	.183	.244	.400	.314
	V	.95	.66	.922	.92	.99	.995
P	*T*	.454	.529	.491	.546	.545	.546
	V >	.69	.22	—	.69	.68	.845
	r	−.210	.093	−.047	.054	.222	.139
	V	.90	.63	.637	.61	.89	.855

For graphical representation, we find the mean of Heavier and Lighter for each of the 28 pairs in the *h*-series (h_L and h_H) and the *P*-series (P_L and P_H), and then draw a straight line (by least squares) through every set of 28 means; the initial (first) and final (28th) figures, when so adjusted, are as follows:

TABLE IX

	P		*h*	
	Initial	Final	Initial	Final
II	.63	.59	.113	.124
VIII	.59	.71	.105	.122

These figures appear graphically in Fig. 2 under II*b* and VIII, respectively.

As for the two observers, we may note that Urban's own investigation terminated in 1907 and Fernberger's began in 1912; five years thus elapsed between the data for II*a* and

II*b*. During this interval, we are told, Obs. II (Urban) "beteiligte sich zwar an psychologischen Untersuchungen aller Art" but did little with lifted weights; so that "man von einer mehr oder weniger vollständigen Unterbrechung der speziellen Übung auf diesem Gebiete sprechen kann"; VIII, on the contrary, "besass am Anfang der Versuchsreihe zwar eine allgemeine psychologische Übung, war jedoch mit den speziellen Aufgaben bei den Versuchen mit gehobenen Gewichten nicht vertraut" (*11*, 293). From this characterization we may assume that VIII is an Obs. who is fairly conversant with the general technique of observing, but without any specialized training in the field of lifted weights; whence we may expect him to begin with high normality and low precision.

The tables above reveal little that is new but do confirm inductions already set forth. With II, as before, goodness of fit (*P*) declines a little as practice goes on; but the recession is quite insignificant and merely proves (as noted hitherto) that when *P* begins high it remains virtually constant even though precision be steadily mounting. The mean level of *P* is about 6 units higher than in II*a* (.61 *vs*. .55) and of *h* some 13 points higher (.118 *vs*. .105).[9] Precision keeps on improving though more slowly than before; he is obviously reaching his limit. Obs. VIII begins pretty low in proficiency (though not extremely so: $h = .105$), but shows good normality from the start ($P = .59$). He well exemplifies the careful Obs., moderately conversant with the methodology of good observing, who distributes his responses normally even though his technical proficiency be still far below its ultimate level. *P* registers some improvement as time passes though it can not be called reliable. From these data, in brief, two generalizations (in line with earlier ones) may with confidence be formulated: (*a*) Fit may run high (above .60) even though precision be but mediocre (.110 to .120). Neither II nor VIII has really low precision (II*a* and Brown) nor yet specially high (III); it may be called average or fair; but their mean degree

[9] A beautiful example of motor retention over a long period with no appreciable diminution; with little practice during the interim, II begins just .003 below his level five years before (.113 *vs*. .116; see Fig. 2, II*a* and II*b*).

of normality is distinctly good. (*b*) *P* when high does not appreciably change though *h* be steadily rising. In precision, VIII improves unquestionably (over 3.5 p.e.) while II rises appreciably (about 2.2 p.e.); in neither case does *P* change reliably.

We come finally to the data from Brown's study of lifted weights. There are four sets, two of five units and two of four. All inter-comparisons must be confined to a given set; so that we have, by our usual method of finding the magnitude of change from each value to all that follow it, ten changes (+ or −) in each of the two five-groups and six in each of the fours: 32 in all. Combining the results for these 32, we have the following proportions of increase:

TABLE X

	Increase	Validity exceeds
P	.523	—
h	.732	.886

If we want a rough measure of the initial and final values of *P* and *h*, for graphical purposes, we may take the mean of the first two figures in each of the four sets for *P* and for *h* as the representative initial value and the mean of the last two as the final value:

TABLE XI

	Initial	Final
P	.70	.73
h	.093	.110

These figures are graphically presented in Fig. 2 under *B*.

About the observer's degree of practice we are told little; merely that she was 'without previous experience in this kind of experiment' (*2*, 8). This seems to imply that, like VIII, she was conversant with the general technique of observing, but had no specialized skill. Her figures merely serve to confirm what has already been noted before. *P* opens higher (.70) and *h* lower (.093) than with any of the other Obs. in weights; the former is unaffected by practice, and while the rise in precision is not technically reliable (the number of cases is small), it is distinctly marked, there being

almost three times as much increase as decrease (.732 *vs.* .268). She is just like the other Obs. who open with low proficiency and high fit: *P* remains constant and *h* rises rapidly during practice.

In view of the whole body of evidence, comprising four independent sets of data, from seven observers, in two types of material (temperature and lifted weights), with unlike procedures in the latter group (Brown *vs.* Urban-Fernberger), the following conclusions are proposed for the reader's appraisal (*cf.* Fig. 2);

(*A*) *How index of normality, P, correlates with measure of precision, h*

(1) When *h* is low (say, below .130), *P* may be high (say, above .50; II*a*, II*b*, VIII, *B*) or low (below .50; I, III). It seems however to be always high with Obs. of known competence and extensive general training (II*a*, II*b*, *C*).

(2) When *h* is high (above .130), *P must* be, in the mean, high (III, perhaps also *C*). Maximal precision is in theory possible only when observed and adjusted frequencies absolutely coincide (perfect fit); this appears from the analysis above.

(*B*) *How P correlates with practice (temporal position)*

(3) When *P* begins high, it remains high, not rising or falling materially as practice proceeds (II*a*, II*b*, VIII, *B*, *C*). The reason is clear: when *P* opens near .60 it cannot rise very far; however competent the Obs. may be, he can hardly remain consistently above .75. There is but little room for improvement, then, when *P* opens high; and of course there is no discernible reason for it to decline (except from temporary fatigue or staleness).

(4) When *P* begins low, it clearly improves as practice proceeds. This we ascribe to: (*a*) the training Obs. gets in the general technique of good observing; (*b*) the acquisition of skill (*h*) in a particular function; as *h* improves, it drags *P* along. This effect is obscured when *P* is high

from the start; but when P opens low, its operation is unmistakable (III and I).

The above conclusions may need scrutiny and revision to accommodate new evidence when it appears; but the facts we have are so clear and accordant in purport and trend, and confirm so well the demands of theory at every stage, that these formulations, so far as they go, may—as we venture to believe—be expected to hold without substantial modification. The problem in any case has not turned out a 'wandering fire'; for as soon as we engage the question with care, instead of denouncing it as unworthy of investigation, it yields a convincing and reasonable answer.

Insofar as practice means high proficiency, it connotes high normality as well. This generalization we construe to mean that as the organism works up to the ultimate levels of performance, as the determinants of its behavior come to be stabilized and refined, as the whole process culminating in a differential judgment is better controlled, its responses come to distribute with approximate normality. $\Phi(\gamma)$ would then by logical implication seem to be the *limiting curve* which our performance tends to describe in at least one extensive and important field of controlled activity (differential sensitivity); even as, with increasingly refined conditions, $V = 1/2\ gt^2$ is the limiting formula for the course of a falling body: a formula never in practice completely attained but always approached. A careful and competent Obs. achieves a high degree of fit in the early stages of practice; and if for any reason it begins low, goodness of fit is found to improve insofar as proficiency betters at all.

Whether the inductions above proposed hold for all cases of practice-change is a wider problem, of course, than our evidence alone can solve; but they may well cover not 'psychophysics' alone but wide regions of quantitative psychology as well, wherever practice takes a rôle. In any event we here have optimal conditions for examining the whole matter, for the reason that we have *known units of stimulation* with which to deal. In so many psychological problems (as,

educational and alertness scales) a $\Phi(\gamma)$-curve is postulated to begin with and the difficulty of a given task or question (its 'stimulus-value') then related to that assumption. Here, on the contrary, no assumptions of any kind are used; the stimulus values can be measured and controlled with any desired degree of precision, and a set of judgment-frequencies thereby derived under optimal conditions of control. That these empirical frequencies constitute, with growing exactitude as practice proceeds, a normal curve, is we think a matter of the first consequence for quantitative theory in psychology; it implies that the performance of an organism, when freed of casual and irregular determinants, tends to approach a standard type.

XII. On the *T*-method: a Procedure for Finding the Trend of any Series of Numbers

In the article (XI) just preceding we have had occasion to use a simple device for discovering the trend of an irregular series of numbers. What we may call the *T*-method (from its dependence upon Tchebycheff's criterion) seems to enjoy certain advantages over the procedures commonly used. As opposed to the correlation coefficient, the *T*-method, being extremely general, may be used with all sorts of distributions, no matter how widely they depart from the gaussian form, whereas r is reliable only when the correlated series are closely normal; it may be used with series too brief to warrant the product-moment procedure; and its criterion of significance does not presuppose normality as does the p.e. of r. The least-square procedure, furthermore, becomes laborious with more than a few observation equations, while its reliability is cumbrous and difficult to determine; whereas the *T*-method is simple and short, and its validity can be found in a moment.

Given a set of data like the 28 figures for P and for h, Obs. II, lighter.[1] We may ask two questions: (*a*) Does P (goodness of fit) tend to increase as trials proceed? (*b*) Do P and h tend to rise and fall together? In reply to (*a*), we count the number of increases from every figure to all the

[1] Article XI, just preceding.

following; clearly if certain effects are due to practice or to rank in a serial order, any one value will be conditioned by all which precede and will in turn affect all which follow. We find now that the first P (.953) is exceeded by two of the remaining 27 (.963 and .995), the second P by one of the later 26, and so on; in a total of 378 possibilities ($27 + 26 + \ldots + 1$), we thus have 200 increments, a ratio of 200/378 or .529, as compared with an expected ratio of .500 in a random series of numbers.

The question now appears: What is the variability of this ratio (.529)?

Its s.d. may be found by resorting to the usual urn-schema, a white ball denoting an increment (p) and black a decrement (q); $p + q = 1$. So long as p and q remain constant, we have what is called a Bernoulli series, in which case the s.d. of p equals $\sqrt{pq/n}$; here we have instead a change in the likelihood of an increase (a white draw) at every comparison; the situation alters from trial to trial so that no two probabilities can be assumed the same. In the present case, then, we have a Poisson series, where the proportion of white and black varies from draw to draw. In this type of situation the s.d. is bound to be smaller than in the Bernoullian and can be determined only if the value of p is given for each successive draw; the Poisson σ is therefore indeterminable with an experimental situation like the present, where we know nothing about the likelihood of an increase (white ball) from trial to trial, but only the empirical proportion derived from the whole set of 378 comparisons or 'draws.' Our only recourse, then, is to use the Bernoullian sigma; in so doing we at least know that any conclusions based upon it will be conservative and a fortiori true of the Poisson situation. Solving $\sqrt{.529 \times .471/378}$, we have $\pm .0257$ as s.d. of .529.

Does the empirical ratio (.529), then, depart significantly from the 'chance' ratio of .500? In answer, we cannot resort to the customary criteria of significance, which are all based upon a normal frequency-surface. There are but two abscissas, two possibilities of increase in our distribution, zero and unity; the base runs from 0 to 1, the zero-ordinate being

178, the unit-ordinate 200, the mean .529; the curve is far indeed from gaussian, being a tall, narrow trapezoid. To show how widely the two forms of distribution differ: normally over 99.7 per cent. of the whole area is included within ± 3 s.d. of the mean; here only about 16 per cent. Otherwise put, in a normal surface all the values composing the mean lie within 3 to 4 s.d. of it; here every value is either 0 or 1 and thus falls 18 to 20 σ from the mean .529. In Tchebycheff's criterion, fortunately, we have a thoroughly general device for measuring significance quite apart from the form of distribution; by this theorem the probability that a given proportion (.529) will deviate positively or negatively from the expected (a priori) value (here .500; in general the mean ratio of increases with a large number of random series of 378 each) by *more* than t times its s.d. is *less* than $1/t^2$. In our case $1/t^2 = .0257^2/.029^2 = .78$; the chance that with 378 comparisons between numbers in random order the proportion of increases will deviate from the expected value of .500 by *more* than ± .029 is *less* than .728, or that the proportion will fall within the range .500 ± .029 is *greater* than .22. If, for purposes of comparison, we now correlate P with the rank-order (the first P, .953, with 1, the second P with 2, and so on for the 28 cases), we get r equal to .093 with s.d. ± .187; the probability that 28 numbers chosen at random would correlate with their serial order more than ± .093 is about .62 (as compared with the more general T-criterion: *less* than .78). It should be noted that the criterion ($1/t^2$) applies only when t is not less than unity.

In like manner, we find (*b*) whether P varies with h by counting how often a rise (or fall) in P is attended by a rise (or fall)in h, or per contra how often one rises when the other falls. In all, P varied with h 242 times in 378, a ratio of .640 with s.d. ± .0247; whence we have $1/t^2$ equal to $.0247^2/.140^2 = .031$. Upon correlating P with h for the 28 cases, we get .475 with s.d. ± .146. Hence by the T-criterion the likelihood is *less* than .031 that two series of 28 numbers written at random would rise and fall together in more than the stated proportion (.500 ± .140) of cases; whereas by the r-method,

the corresponding probability that two random series of 28 numbers would yield an r as high as .475 is about .0013. It will be noted that the chances favoring validity are higher by the r-method in both cases. The T-procedure is conservative in two ways: (*a*) we are forced to use a s.d. (Bernoullian) which is invariably larger than the true (Poisson) value; (*b*) Tchebycheff's theorem states the probability always in terms of greater (or less) than a given fraction, the precise degree of excess or defect being of course unknown. Apart from the general conservatism of T, we noted above (Table VIII) how nearly the two methods agree in the conclusions they justify and the reliability they afford.

With a limited number of cases it is well to take the *amount* of change (+ or −) from each value to all that follow it instead of merely counting the *number* of changes up and down; in a long series the mean positive may be expected to equal the mean negative change, but in a short series this cannot be assumed. Thus, with Obs. III, Lighter, h rises 1.455 in 25 increases and falls .391 in 11 decreases: a proportion of 1.455/1.846 or .788 in magnitude and 25/36 or .694 in number —an appreciable discrepancy. Of the two the former is clearly better.

In like manner, we may determine whether P correlates with h by finding *how much* P changes when moving with h and how much when moving against h. So in III, Lighter: when h is increasing, P registers a total increment of 11.59 and a decrement of 1.35; when h is falling, P rises 1.46 and drops 2.95. P thus varies with h in the sum of 11.59 + 2.95 and versus h by 1.35 + 1.46; whence the proportion of increase to the whole extent of change is 14.54/17.35 or .832; the reliability of which is found in the usual way.

To sum up: in the T-method, here demonstrated by means of concrete examples, we have a simple and convenient device for analyzing any body of material. Its cardinal advantage over the common modes of treatment lies in its generality, its universal applicability; it can be used with all manner of distributions, though they depart widely from the gaussian form; the question of 'normality' does not arise. The

Pearson r is, of course, a more precise measure of relation within the special conditions for which it is designed (two normal distributions); and the least-square procedure is useful in fitting a straight line or other curve to a set of figures; but the T-method seems worth using as a measure of relation whose reliability (unlike least-squares) [2] can always be readily found; and which, though less refined than the product-moment r, is far more widely applicable.

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[2] In fact, the writer is not aware of any procedure, generally usable, for determining the reliability of the slope of a fitted line.

SPECIFIC SERIAL LEARNING; A STUDY OF BACKWARD ASSOCIATION

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Preliminary Statement

Theory of remote associations.—For some years it has been assumed that when associations are formed in a series from item *1* to item *2*, from *2* to *3*, from *3* to *4*, etc., certain remote associations are also formed; so that item *2*, for example, becomes connected with all of the other items in the series, in both the forward and backward directions. Ebbinghaus carried out the original experiments on which this theory of remote associations was based; and he claimed that the remote associations are stronger in the forward than in the backward direction, and that the strength of the associations decreases with the number of intervening items.[1] About two years ago the writer suggested some of the difficulties with this theory expecially as it relates to backward associations.[2] The present paper describes experiments in which the problem of backward association is for the first time submitted to a systematic experimental investigation.[3]

[1] H. Ebbinghaus, *Memory, a contribution to experimental psychology*, 1885, 90–117 (Tr. by Ruger and Bussenius). See also G. E. Müller & F. Schumann, Experimentelle Beiträge zur Untersuchungen des Gedächtnisses, *Zsch. f. Psychol. u. Physiol. d. Sinnesorgane*, 1894, **6**, 81–190, 257–339; G. E. Müller & A. Pilzecker, Experimentelle Beiträge zur Lehre vom Gedächtniss, *Zsch. f. Psychol. u. Physiol. d. Sinnesorgane, Ergänzungsband* I. (1900), pp. 1–288; A. Wohlgemuth, Ueber die Richtung der Assoziationen, *Ber. über d. V. Kongress f. exper. Psychol.*, 1912, 284–287, and On memory and the direction of associations, *Brit. J. Psychol.*, 1913, **5**, 447–465; A. Michotte & F. Fransen, Note sur l'analyse des facteurs de la mémorisation et sur l'inhibition associative, *Ann. de l'institut sup. de philos.* (Université de Louvain), 1914, **3**, 503–549; H. A. Carr & A. S. Freeman, Time relationships in the formation of associations, *Psychol. Rev.*, 1919, **26**, 465–473.

[2] H. Cason, The concept of backward association, *Amer. J. Psychol.*, 1924, **35**, 217–221.

[3] I wish to make a grateful acknowledgment to my wife, Eloise Boeker Cason, for much valuable assistance in carrying out the experiments and in the preparation of the manuscript, and to all those students at Syracuse University who, as subjects, gave so generously of their time and interest.

Problem.—If the series *1-2-3-4-5*, etc., is learned so that the items can be reproduced in the proper order, does this learning result in the formation of backward associations and so facilitate the later learning of the items in the order *5-4-3-2-1*? When one individual learns the items in the order *1-2-3-4-5* this learning is of course in the *forward* direction; and when another individual, or the same individual at a different time, learns the same items written in the order *5-4-3-2-1* this learning is likewise in the *forward* direction. It follows that if there is some practice in the order *5-4-3-2-1*, while the principal task of learning the series in the order *1-2-3-4-5* is in progress, then both kinds of learning are really *forward* in nature. The theory of backward associations assumes that when associations are formed in the order *1-2-3-4-5*, associations are at the same time established in the order *5-4-3-2-1*. No serious attempts have been made to explain just how or why these backward associations are formed, although the question is of special significance for the psychology of learning.

PART I. TESTS WITH ROTE MATERIAL

Introduction

Suggested test with the letters of the alphabet.—We thought at first that the letters of the alphabet might be used as learning material. The subjects have already established strong forward associations between these items, and the proposed test might be made as follows. Derive 2 lists from the letters of the alphabet, one list being made up of the adjacent letters arranged in the backward direction, and the other list being made up of the same letters arranged in a mixed order. The theory of backward association might be tested by comparing the relative difficulty of learning the derived backward list and the derived mixed list. The superiority of the backward list would indicate the presence of backward associations, etc. On second thought, however, it seems that such results would not be conclusive, on account of several complicating factors which may be described as follows.

Standards of comparison for backward association.—A subject who is familiar with the letters of the alphabet has already established a variety of associations among the pairs and groups of letters; and a derived mixed list would not necessarily represent zero learning at the beginning of a new learning period. The situation affords the following possibilities. (1) The mixed list might include some strong forward associations between the pairs of letters, which would give it a place *above* zero at the beginning of a new learning period. (2) The mixed list might include some associations which would interfere with the new learning of the same material, and this circumstance would give it a place *below* zero at the beginning of a new learning period. (3) It is also possible that the facilitating and inhibiting associations in the mixed list might just balance each other, and in such an event the later learning of the letters in the mixed order would begin at a zero level. In the simple procedure with the letters of the alphabet, it is not known whether the new learning of the mixed list begins above zero, below zero, or at zero, and a comparison between the mixed list and the backward list would in no way indicate the presence or the absence of backward associations.

The best type of material for the purposes of the present experiment seems to be that which contains very few associations between the separate pairs or groups of items. There should be a large number of items, and they should be practically equal in difficulty. Series of letters are unsatisfactory as material because of the small number of letters in the alphabet. The same objection applies to the use of digits. Most of the difficulties suggested above can be satisfactorily met or avoided by a general plan which is outlined in the following paragraphs.

Procedure with forward, backward, and separated lists.—Let us suppose that each of the figures in Table I represents a different nonsense syllable. The following plan and procedure can then be used.

(*a*) Each of the 12 horizontal rows of syllables constitutes a *practice list*, and each practice list is typewritten on a separate

card. The subject is required to learn these lists singly. The learning is limited as far as possible to the formation of associations in the forward direction, and each list is learned to as nearly the same degree of proficiency as possible. The lists are rotated from time to time for a group of subjects, so that each list has the same advantages on account of its position in the whole set of material.

TABLE I

PROCEDURE WITH FORWARD, BACKWARD AND SEPARATED LISTS

F_1	1	2	3	4	
	11	12	13	14	B_1
	21	*22*	*23*	*24*	
F_2	31	32	33	34	
	41	42	43	44	B_2
	51	*52*	*53*	*54*	
F_3	61	62	63	64	
	71	72	73	74	B_3
	81	*82*	*83*	*84*	
F_4	91	92	93	94	
	101	102	103	104	B_4
	111	*112*	*113*	*114*	
	S_1	S_2	S_3	S_4	

(*b*) The 48 syllables represented in Table I also constitute a different set of 12 lists, which will be referred to as the *test lists*. The test lists are of 3 kinds: 4 *forward* lists, 4 *backward* lists, and 4 *separated* lists. The *forward* lists are made up of the syllables in the rows directly to the right of the '*F*'s' in the table, the items being arranged in the order shown, as *1-2-3-4*, *31-32-33-34*, etc. The *backward* lists are made up of the syllables in the rows directly to the left of the '*B*'s' in the table, the items being arranged and written in the reverse order, as *14-13-12-11*, *44-43-42-41*, etc. The *separated* lists are made up of the syllables in the columns above the '*S*'s,' and these items are represented by the italicized numbers, and are arranged from bottom to top, as *111-81-51-21*, *112-82-52-22*, etc.

(*c*) The 12 test lists derived from the syllables represented in Table I also constitute 4 groups of 3 test lists each. Each

group of lists is made up of a forward list, a backward list, and a separated list, as F_1, B_1, S_1, etc. For the group of subjects, the 4 syllables in each list, the 3 lists in each group, and the 4 groups in the whole set of material are rotated from time to time so that no syllable, list, or group has any advantages on account of its position. Any effects due to practice, fatigue, and changes in attitude are equalized in this way for the *F*-, *B*-, and *S*-lists.

(*d*) The subject learns each practice list to the point of one correct recitation, and then learns each test list to the same degree of proficiency. Nothing is said about the purpose of the experiment, or the method of making up the test lists from the same syllables used in the practice lists. The subject is simply instructed to learn one list at a time, and is urged to exert the same maximal effort in learning all of the lists.

(*e*) When the procedure for an individual subject is completed, it is possible to make a comparison between the efficiency of learning the *F*-, *B*-, and *S*-lists. It is to be expected that the *F*-lists will be superior to the *B*-lists or the *S*-lists, because the syllables making up the *F*-lists were previously learned in the same order in the practice lists.

(*f*) The syllables in both the *B*-lists and the *S*-lists are arranged in the *backward direction*, when they are compared with the arrangement of the same syllables in the original set of practice lists. There is this difference, however, that in the *B*-lists the same syllables are still adjacent to each other, and no other syllables intervene, whereas in the *S*-lists the syllables are separated at some distance from each other, and about 11 syllables intervene. Each syllable in a single *S*-list was also on a different card in the practice lists. If the learning of the practice lists does result in the formation of backward associations, these backward associations should be much stronger between syllables which are adjacent to each other than between syllables which are separated from each other by *11* intervening syllables.

A significant feature of the above procedure is that the learning of the practice lists does not facilitate the later learning of the *S*-lists, which *begins at zero*. The efficiency of learning the *S*-lists may be considered as a zero base of learn-

ing; and it may be compared with the efficiency of learning the *B*-lists.

The subject's familiarity with each of the individual syllables naturally increases when he learns the practice lists, and this gain in familiarity facilitates the formation of associations in the test lists. This facilitating effect, however, is exactly the same for the *F*-, *B*-, and *S*-lists, and does not prejudice the comparisons which are made.

We have now briefly outlined the plan of the first experiment of the present paper. Although the general arrangement of the material was the same as that described in connection with Table I, the number of syllables, lists, and groups of lists was larger than has been indicated.

Subjects.—The 62 different subjects of the present paper were college students of both sexes, in the sophomore, junior, and senior classes. Very few of the subjects were familiar with the theory of backward association, and no subject suspected that this was the problem being investigated. The writer acted as the experimenter on all occasions. All of the 31 subjects used in Part I were taking the writer's course in abnormal psychology at the time the experiment was carried out.

Material

It appears from our discussion that the material should be made up of a large number of items, and the items should be practically equal in difficulty. We have attempted to approach this ideal condition in the preparation of a set of 300 *nonsense syllables*, which are shown in Table II.

In order to make a more careful analysis of the learning situation, a second set of material was made up of 300 short *familiar words*, which are given in Table III.[4] It should be

[4] The following method was used in forming the list of short familiar words. All of the common 3-letter words in Thorndike's *Teacher's Word Book* (1921) were first used to form a preliminary list. The following classes of words were then eliminated: (1) All proper names; (2) all words of more than one syllable; (3) some of the words which would be distracting in a learning experiment; (4) all words having a credit-number of 3 or 4 (a small credit-number in Thorndike's list indicates that the word is relatively uncommon); (5) some of the words with credit-numbers between 4 and 10; and (6) other words which seem to be fairly uncommon. The above classes were eliminated until the list had been reduced to the 300 words which are shown in Table III.

TABLE II

NONSENSE SYLLABLES (300) FOR LEARNING EXPERIMENTS

BAF	DUS	GEX	HUV	MAF	NUP	SEB	TUS	VUG	ZAN
BAP	DUT	GEZ	HUX	MAV	NUV	SEG	TUV	VUL	ZAR
BAV	DUV	GID	HUZ	MEF	PAB	SIB	VAB	VUM	ZAX
BAW	FAP	GIF	KAX	MEP	PAF	SOF	VAD	VUP	ZED
BAZ	FAV	GIK	KAZ	MEV	PEB	SOG	VAF	VUR	ZEF
BEM	FAW	GIR	KEB	MEZ	PEF	SOV	VAG	VUT	ZEG
BEP	FEG	GIS	KEF	MIB	PEM	SOZ	VAM	VUX	ZEM
BEV	FEK	GIX	KEM	MIG	PIB	SUF	VAP	VUZ	ZEN
BEX	FEP	GOK	KES	MIP	PIM	SUG	VAW	WAB	ZEP
BIV	FEV	GOM	KET	MIV	PIV	SUL	VEB	WEF	ZER
BIX	FEX	GOS	KEV	MOF	POB	SUV	VED	WEM	ZEV
BOF	FID	GOX	KEX	MOG	POF	TAF	VEF	WEX	ZIB
BOP	FIK	GUB	KIB	MOV	POG	TAV	VEP	WEZ	ZID
BOV	FIM	GUK	KIF	MUB	POV	TAY	VIB	WIB	ZIF
BUP	FIP	GUP	KIG	MUP	POY	TAZ	VID	WIM	ZIL
BUV	FIV	GUR	KIP	MUX	POZ	TEB	VIG	WIX	ZIR
DAF	FOD	GUX	KIR	NAD	PUM	TEF	VIK	WOB	ZIV
DAG	FOS	GUZ	KIV	NAF	PUV	TEG	VIP	WOG	ZOF
DAP	FOV	HAB	KUV	NAR	PUZ	TEK	VIX	WOL	ZOM
DAV	FOY	HAR	KUX	NAZ	REB	TEP	VOB	WOX	ZOR
DAX	FUB	HEB	LAZ	NEF	RET	TEV	VOD	WUB	ZOV
DEG	FUP	HEF	LEB	NEG	REZ	TEY	VOF	WUD	ZOY
DEV	FUV	HEG	LEM	NEP	RIK	TIB	VOK	WUG	ZUB
DEZ	GAW	HES	LEZ	NIM	RIV	TIF	VOM	WUK	ZUD
DIB	GAX	HEX	LIG	NIV	ROF	TIV	VOP	WUP	ZUF
DIZ	GEB	HIB	LIS	NIZ	RUK	TOF	VOY	WUT	ZUG
DOB	GED	HIF	LOY	NOF	RUP	TOV	VOZ	WUV	ZUL
DOY	GEK	HIG	LOZ	NOY	RUV	TOZ	VUB	WUX	ZUR
DUF	GEP	HUD	LUB	NUG	RUX	TUD	VUD	ZAB	ZUT
DUR	GEV	HUK	LUF	NUK	SAB	TUP	VUF	ZAK	ZUV

The first step in the preparation of the nonsense syllables was to form all combinations of 3 letters in the alphabet, with a vowel (*a*, *e*, *i*, *o*, or *u*) between 2 different consonants. After several thousand combinations had been formed in this way, the following classes were eliminated:

(*a*) All syllables which are difficult to pronounce. This class includes the syllables containing *q*, those beginning with *x* or *y*, and those ending in *eh*, *ih*, *oh*, *uh*, *j*, *iw*, and *uw*.

(*b*) All syllables which are ambiguous in sound. This class includes all syllables containing *c*, because of the similarity in sound to *k* or *s*; and those syllables beginning with *j*, because of the similarity in sound to g. In the case of 2 syllables ending in *ah* and *ar*, only one of the syllables was retained.

(*c*) All syllables which readily become meaningful. For example, *bef* was eliminated because it can be pronounced 'beef,' *dit* because it is a familiar French word, *gop* because of its association in American politics, *tex* because it is an abbreviation for Texas, *pif* on account of the slang usage, etc. After the writer had eliminated a large number of syllables by this criterion, another judge assisted in making further eliminations until the list had been reduced to 500 syllables.

(*d*) All syllables which become meaningful after a brief examination. Before the final eliminations were made, the remaining 500 syllables were alphabetically arranged and were submitted to several qualified persons for their judgments. The following method was used. The judge looked at each syllable, pronounced it in

various ways, and thought about it carefully, etc. He then gave any meaningful associations which he was able to obtain, and the writer kept a record of the number of meaningful associations for each of the 500 syllables. On the basis of this group judgment, the list was finally reduced to the 300 syllables which are given in Table II.

These syllables have the following advantages as learning material: (1) They have very little meaning for the average person, and are more or less equal in difficulty; (2) All of the letters in each syllable can be easily pronounced, and very few of the syllables are ambiguous in sound; (3) The small number of letters in each syllable makes it possible to use a large number of items in a given learning period, and the general similarity in the form of the syllables facilitates the scoring. It may be added that about half of the items in Table II are *words* which can be found in a large dictionary; but these items make very good material for learning experiments since they do not readily become meaningful for the average person.

It may be interesting in this connection to compare the method used by Müller and Schumann, *op. cit.*, pp. 99–106. See also E. A. M. Gamble, A study in memorizing various materials by the reconstruction method, *Psychol. Monog.*, 1909, **10**, no. 43, 18–23.

Table III

Familiar words (300) for learning experiments

ACT	BIB	DIE	FOB	HOT	LET	OAK	POP	SEA	TIP
ADD	BID	DIG	FOE	HOW	LID	OAT	POT	SET	TOE
AGE	BIN	DIM	FOG	HUE	LIE	ODD	PRY	SEW	TON
AID	BIT	DIN	FOR	HUG	LIP	OFF	PUG	SHE	TOO
AIL	BOG	DIP	FOX	HUM	LIT	OIL	PUP	SHY	TOP
AIM	BOY	DOG	FRY	HUT	LOG	OLD	PUT	SIN	TOY
AIR	BUD	DOT	FUN	ICE	LOT	ONE	RAG	SIP	TRY
ALL	BUG	DRY	GAG	ILL	LOW	ORB	RAM	SIR	TUB
AND	BUN	DUE	GAS	INK	MAN	ORE	RAP	SIT	TUG
ANT	BUS	DUG	GAY	INN	MAP	OUR	RAT	SIX	TWO
APE	BUT	EAR	GEM	ITS	MAR	OUT	RAW	SKY	URN
APT	BUY	EAT	GET	JAM	MAT	OWE	RAY	SLY	USE
ARE	CAB	EBB	GOT	JAR	MAY	OWL	RED	SOB	VAN
ARK	CAN	EGG	GUM	JAW	MEN	OWN	RIB	SOD	VEX
ARM	CAP	ELM	GUN	JET	MEW	PAD	RID	SON	VOW
ART	CAR	END	HAD	JIG	MID	PAN	RIG	SPY	WAG
ASH	CAT	ERR	HAM	JOB	MIX	PAT	RIM	SUM	WAR
ASK	COB	EVE	HAS	JOY	MOW	PAW	RIP	SUN	WAS
ATE	COD	EYE	HAT	JUG	MUD	PAY	ROB	TAG	WEB
AWE	COO	FAN	HAY	KEG	MUG	PEG	ROD	TAN	WED
BAD	COT	FAR	HEM	KEY	NAP	PEN	ROT	TAP	WET
BAG	COW	FAT	HEN	KID	NAY	PER	ROW	TAR	WHO
BAN	COY	FED	HER	KIN	NET	PET	RUB	TAX	WHY
BAR	CRY	FEE	HID	KIT	NEW	PEW	RUG	TEA	WIG
BAT	CUP	FIG	HIP	LAD	NOD	PIE	RUN	TEN	WIN
BAY	CUR	FIN	HIS	LAG	NOR	PIG	SAD	THE	WIT
BED	DAY	FIR	HIT	LAP	NOT	PIN	SAP	THO	WOE
BEE	DEN	FIT	HOE	LAY	NOW	PIT	SAT	THY	YES
BEG	DEW	FIX	HOG	LED	NUN	PLY	SAW	TIE	YET
BET	DID	FLY	HOP	LEG	NUT	POD	SAY	TIN	YOU

noted that the syllables given in Table II have very little meaning and are quite unfamiliar, whereas the words in Table III are all in common use and are the most familiar 3-letter words that can be found.

Procedure

We may now complete the description of the procedure which we have devised. Table IV shows the arrangement of one set of practice lists and test lists. The 108 nonsense syllables which are represented by symbols were taken by chance from the syllables given in Table II.

Table IV

THE 18 PRACTICE LISTS AND THE METHOD OF CONSTRUCTING THE 18 TEST-LISTS FROM THE SAME SYLLABLES

Syllables for the forward, backward and separated lists	Order for learning the 6 *F*- and the 6 *B*-lists	The 18 practice lists (each on a horizontal line). The 18 test lists are constructed as follows: 6 *F*-lists reading from left to right; 6 *B*-lists from right to left, and 6 *S*-lists from bottom to top						Order for learning the 18 Pract. lists
F		2*F*1	2*F*2	2*F*3	2*F*4	2*F*5	2*F*6	1
B	2	2*B*6	2*B*5	2*B*4	2*B*3	2*B*2	2*B*1	2
(*S*)		2*S*6	4*S*6	1*S*6	5*S*6	3*S*6	6*S*6	3
F		4*F*1	4*F*2	4*F*3	4*F*4	4*F*5	4*F*6	4
B	4	4*B*6	4*B*5	4*B*4	4*B*3	4*B*2	4*B*1	5
(*S*)		2*S*5	4*S*5	1*S*5	5*S*5	3*S*5	6*S*5	6
F		3*F*1	3*F*2	3*F*3	3*F*4	3*F*5	3*F*6	7
B	3	3*B*6	3*B*5	3*B*4	3*B*3	3*B*2	3*B*1	8
(*S*)		2*S*4	4*S*4	1*S*4	5*S*4	3*S*4	6*S*4	9
F		6*F*1	6*F*2	6*F*3	6*F*4	6*F*5	6*F*6	10
B	6	6*B*6	6*B*5	6*B*4	6*B*3	6*B*2	6*B*1	11
(*S*)		2*S*3	4*S*3	1*S*3	5*S*3	3*S*3	6*S*3	12
F		1*F*1	1*F*2	1*F*3	1*F*4	1*F*5	1*F*6	13
B	1	1*B*6	1*B*5	1*B*4	1*B*3	1*B*2	1*B*1	14
(*S*)		2*S*2	4*S*2	1*S*2	5*S*2	3*S*2	6*S*2	15
F		5*F*1	5*F*2	5*F*3	5*F*4	5*F*5	5*F*6	16
B	5	5*B*6	5*B*5	5*B*4	5*B*3	5*B*2	5*B*1	17
(*S*)		2*S*1	4*S*1	1*S*1	5*S*1	3*S*1	6*S*1	18
Order for learning the 6 *S*-lists.....		2	4	1	5	3	6	

Arrangement of the material.—The syllables in the table are arranged in 18 horizontal rows, with 6 syllables in each row. The order in which the subject learned these rows of syllables, or practice lists, is shown in the right-hand column of the table.

Each symbol in the table is constructed with reference to the arrangement or position of the syllables in the 18 *test* lists. Thus the symbol '2*F*6' represents the 6th syllable in the 2nd forward list. The first digit in each symbol also indicates the group of test lists to which the syllable belongs. The order in which the 6 groups of test lists were taken is shown in the 2d column (for the *F* and *B* lists) and in the bottom row (for the *S* lists).

Preliminary practice.—The subject is given a *preliminary practice list* and is instructed to study it by the whole method until he can make a correct oral recitation. He reads the list aloud at a fast but efficient and comfortable rate. He is urged to pronounce the syllables accurately, and to look at each syllable as he reads it aloud. If a mistake occurs in pronunciation, he passes on to the next syllable with as little delay as possible.

In a learning situation of this kind, the subject occasionally shows a tendency to look away from the list and secretly attempt to recite to himself. The experimenter tries to prevent this private recitation. The subject is instructed to attempt a final recitation for the experimenter only when he is *certain* that he can recite *perfectly*. When the subject is ready, he turns the card which he holds in his hand face-down on the table, and proceeds to recite without giving the experimenter any further warning. His errors are not corrected, and he does not see the card again.

A record is made of the number of repetitions, the total time consumed, and the score on the recitation. In scoring the recitation, a credit of one point is given for each correct letter in the right position in each syllable, but no syllable receives any credit unless it is also in the right position in the list.

The experimenter is supplied with several complete sets of material arranged as described in connection with Table IV but differing from each other in length. The particular set of material is employed which best suits the subject's learning ability.

Practice lists.—The method of learning the practice lists is the same as that described above for the preliminary practice

lists. A brief rest is allowed when all of the practice lists have been learned singly, and after this rest the practice lists are relearned.

Test lists.—The same method is used in learning the *test lists*, except that the subject is allowed to read each test list only a specified number of times before attempting a recitation. The number of repetitions allowed in each trial is partly determined by the subject's performance on the practice lists. Although this number varies from time to time, it is always the same for the *F*-, *B*-, and *S*-lists in a single group of test lists. The subject is instructed to read the test lists at exactly the same speed, and to exert a constant maximal effort in learning all of the lists. A record is made of the number of repetitions of each list, the total time spent in studying or reading, and the score on the recitation.

The subjects in one group were required to study each test list until they could recite it perfectly, and the efficiency of learning was indicated by the total time consumed in learning. The results for this group of subjects are the first to be described in the next section.

The procedure described above has several points of superiority over the method used by Ebbinghaus and others. As may be inferred from our discussion of the proposed test with the letters of the alphabet, the derived 'mixed' lists which Ebbinghaus used did not represent a zero base of learning. The derived 'separated' lists which we used can be considered as a zero base of learning, and fairly used for purposes of comparison. Our procedure is definitely superior to his in regard to the number of subjects, since he was both subject and experimenter in all of his tests. The subjects which we used did not know the method which had been followed in constructing the lists, and they knew nothing about the problem being investigated. Several other advantages of our procedure will be clearly seen in the description of the different types of material and the variations in procedure.

RESULTS

Efficiency of learning measured in terms of the time required to learn lists of nonsense syllables correctly.—In Table V we have summarized the results on the *F*-, *S*-, and *B*-lists for the 10 subjects who used a set of material consisting of either 15 lists of 5 syllables each or 18 lists of 6 syllables each. Relearning the *F*-lists after a short interval required almost 3 sec per syllable, and learning the *S*-lists or the *B*-lists required

between 4 and 5 sec. The lower left-hand portion of the table shows that the superiority of the *F*-lists over the *S*-lists or the *B*-lists amounts to almost 2 sec of learning time per syllable. The lower right-hand portion of the table shows that a slightly longer time was required to learn the *S*-lists than the *B*-lists, and the suggestion is in favor of backward association.

TABLE V

SET OF 15 (OR 18) LISTS OF 5 (OR 6) NONSENSE SYLLABLES EACH. SCORE IS THE TIME (IN SEC) REQUIRED TO LEARN EACH TEST LIST CORRECTLY. (GROSS SCORES ARE CHANGED OVER INTO NUMBER OF SEC PER SYLLABLE.)

10 Ss	Forward	Separated	Backward
Ave.	2.88 ± .17	4.81 ± .30	4.63 ± .28
Med.	2.60 ± .21	4.25 ± .38	4.20 ± .35
S. D.	1.81	3.32	3.05
Meas.	54	54	54

F–*S* (Ave.)	− 1.93 ± .34
F–*S* (Med.)	− 1.65 ± .43
(*F*–*B* (Ave.)	− 1.75 ± .33)
(*F*–*B* (Med.)	− 1.60 ± .41)
S–*B* (Ave.)	+ 0.18 ± .41
S–*B* (Med.)	+ 0.05 ± .52

Efficiency of learning measured in terms of the number of letters correctly reproduced in lists of nonsense syllables after a given number of repetitions.—Table VI shows the results for 9 subjects who used a set of material consisting of 18 lists of 6 syllables each. The maximal score on a single list is 18 letters, or 18. The table shows that, with the same number of repitions, more material can be reproduced in the *F*-lists than in the *S*- or *B*-lists, and that more material can be reproduced in the *B*-lists than in the *S*-lists. The conclusion follows that some backward associations have been formed on account of the previous learning of the practice lists. It should be noted that this result is more reliable than the result represented in Table V.

Table VII shows the results for 7 subjects who used 21 lists of 7 syllables each. The maximal score on each test list is 21. It appears that the previous learning of the practice lists has again resulted in the formation of backward associa-

TABLE VI

SET OF 18 LISTS OF 6 NONSENSE SYLLABLES EACH. SCORE IS THE NUMBER OF LETTERS CORRECTLY REPRODUCED AFTER A GIVEN NUMBER OF REPETITIONS.

9 Ss	Forward	Separated	Backward
Ave.	15.74 ± .27	14.56 ± .24	14.85 ± .23
Med.	16.83 ± .34	15.25 ± .30	15.50 ± .29
S. D.	2.90	2.63	2.47
Meas.	54	54	54

F–S (Ave.)	+ 1.18 ± .36	*S–B* (Ave.)	− 0.29 ± .33
F–S (Med.)	+ 1.58 ± .45	*S–B* (Med.)	− 0.25 ± .42
(*F–B* (Ave.)	+ 0.89 ± .35)		
(*F–B* (Med.)	+ 1.33 ± .45)		

TABLE VII

SET OF 21 LISTS OF 7 NONSENSE SYLLABLES EACH. NUMBER OF LETTERS CORRECTLY REPRODUCED AFTER A GIVEN NUMBER OF REPETITIONS.

7 Ss	Forward	Separated	Backward
Ave.	17.98 ± .30	15.31 ± .27	15.45 ± .31
Med.	18.75 ± .37	14.81 ± .34	16.29 ± .39
S. D.	3.09	2.79	3.21
Meas.	49	49	49

F–S (Ave.)	+ 2.67 ± .40	*S–B* (Ave.)	− 0.14 ± .41
F–S (Med.)	+ 3.94 ± .50	*S–B* (Med.)	− 1.48 ± .52
(*F–B* (Ave.)	+ 2.53 ± .43)		
(*F–B* (Med.)	+ 2.46 ± .54)		

TABLE VIII

SET OF 27 LISTS OF 9 SHORT FAMILIAR WORDS EACH. NUMBER OF LETTERS CORRECTLY REPRODUCED AFTER A GIVEN NUMBER OF REPETITIONS.

5 Ss	Forward	Separated	Backward
Ave.	25.73 ± .25	19.29 ± .45	20.84 ± .51
Med.	26.75 ± .31	18.92 ± .56	21.00 ± .63
S. D.	2.48	4.45	5.03
Meas.	45	45	45

F–S (Ave.)	+ 6.44 ± .51	*S–B* (Ave.)	− 1.55 ± .68
F–S (Med.)	+ 7.83 ± .64	*S–B* (Med.)	− 2.08 ± .84
(*F–B* (Ave.)	+ 4.89 ± .57)		
(*F–B* (Med.)	+ 5.75 ± .70)		

tions. This result is on the whole more reliable than either one of the similar results represented in Tables V and VI.

Efficiency of learning measured in terms of the number of letters correctly reproduced in lists of familiar words after a given number of repetitions.—Table VIII shows the results for 5 subjects who used 27 lists of 9 familiar words each. The words used in this set of material were taken by chance from the words given in Table III. The maximal score on each test list is 27. The table shows that the previous learning of the practice lists has resulted in the formation of some backward associations. This result is more reliable than any one of the results represented in Tables V, VI, and VII.

Time consumed in repeating the test lists.—In the procedure where the efficiency of learning was measured in terms of the number of letters which could be correctly reproduced, the number of repetitions was purposely varied by the experimenter from time to time, but all of the lists in a single group of test lists were repeated the same number of times. The subject was instructed to read all lists at exactly the same speed. In order to check the results more carefully (for the last 14 subjects), a record was made of the actual time spent in reading each test list, and these time scores are given in Table IX. In spite of the careful instructions, less time was

TABLE IX

TIME (SEC) CONSUMED IN REPEATING THE TEST LISTS

14 Ss	Forward	Separated	Backward
Ave.	14.35 ± .39	15.52 ± .41	15.46 ± .43
Med.	13.40 ± .49	14.50 ± .51	14.60 ± .54
S. D.	5.92	6.16	6.53
Meas.	104	104	104

F–S (Ave.)	− 1.17 ± .57
F–S (Med.)	− 1.10 ± .71
(*F–B* (Ave.)	− 1.11 ± .58)
(*F–B* (Med.)	− 1.20 ± .73)
S–B (Ave.)	+ 0.06 ± .59
S–B (Med.)	− 0.10 ± .74

The time of reading was taken in the case of 14 Ss; of these, 5 Ss used 27 lists of 9 familiar words, 5 used 21 lists of 7 nonsense syllables, and 4 used 18 lists of 6 nonsense syllables.

consumed in repeating the *F*-lists than in repeating the *S*-lists or the *B*-lists, but the same time was spent in repeating the *S*-lists and the *B*-lists.

Results of individual subjects.—Although the summaries given in Tables V, VI, VII, and VIII seem to support the theory of backward association, these results are 4 sets of average results for 4 groups of subjects, and they tell little about the performance of the individuals. Comparisons have therefore been made for each of the 31 subjects. Table X shows a sample result.

TABLE X

INDIVIDUAL RESULTS FOR SUBJECT O.F.O., WHO USED 27 LISTS OF 9 FAMILIAR WORDS EACH

Group of Test Lists	No. of Repet'ns	Forward		Separated		Backward	
		Time of Repet'ns	Score (No. of Letters)	Time of Repet'ns	Score (No. of Letters)	Time of Repet'ns	Score (No. of Letters)
1	2	10.0	26	12.2	18	10.6	24
2	2	10.8	27	10.8	27	11.2	18
3	2	9.8	25	11.2	18	10.8	15
4	2	11.2	15	12.4	27	12.6	27
5	2	11.6	27	11.0	19	11.4	15
6	2	10.6	27	12.0	15	11.6	27
7	2	10.4	27	12.2	26	10.6	17
8	2	10.2	27	10.8	24	12.2	21
9	2	10.0	26	9.6	22	9.8	26
Ave.....		10.5	*25.2*	11.4	*21.8*	11.2	*21.1*
Med.....		10.4	*27.0*	11.2	*22.0*	11.2	*21.0*

The *F*-lists are superior to the *S*-lists or the *B*-lists for all subjects. Table XI shows the number of subjects who gave results in favor of backward associations, and the number of subjects who gave the opposite results. If we consider Table XI as a whole, it appears that, in the procedure which we used, backward associations are formed in about 2 cases out of 3. There is a possibility of course that "chance" factors may account for the one individual out of 3 who gives results which are opposed to the theory of backward association,—and the theory may still be true as a general principle. On the other hand, backward associations may be formed in one individual

who uses one method of learning, but not in another individual who uses a different method of learning. The latter alternative seems to be more strongly supported.

TABLE XI

NUMBER OF SUBJECTS WHO GAVE RESULTS IN FAVOR OF, OR OPPOSED TO, BACKWARD ASSOCIATION

	Lists of 5 (or 6) Syl. (Table V)	Lists of 6 Syl. (Table VI)	Lists of 7 Syl. (Table VII)	Lists of 9 Words (Table VIII)	Total
S-Lists Superior	2	4	3	2	11
B-Lists Superior	8	5	4	3	20
No. Subjects	10	9	7	5	31

INTERPRETATION OF RESULTS

While the subjects were learning the practice lists, a definite effort was made to limit the learning process to the formation of associations in the forward direction, so that the conditions of the experiment would be suitable for testing the theory of backward association. When nonsense syllables and disconnected words are used as material, however, it seems to be practically impossible to limit the learning process in this strict manner. The following irregularities and difficulties were observed while the subjects were engaged in the rote learning of the practice lists.[5]

We attempted to observe the learning more carefully by requiring the subject to repeat the items aloud; but the subject's thoughts do not coincide with the oral pronunciation of the items. We also tried to follow the process more closely by observing eye-movements; but when the subject is looking at one item and pronouncing it he is frequently apprehending some other.[6] The fluctuations and irregularities which occur are apparently beyond control.[7] In spite of the most rigid

[5] We have pointed out some of these irregularities in a previous article, The concept of backward association, *Amer. J. Psychol.*, 1924, *35*, 217–221.

[6] *Cf.* Müller & Schumann, *op. cit.*

[7] *Cf.* Gamble, *op. cit.*, pp. 11, 39–43, 102–149, 199–202.

instructions, the subject also makes mistakes in pronouncing the syllables or words. When these mistakes occur, there is a tendency for the subject's eyes and thought processes to shift backwards in the list. It is important to note that these fluctuations constitute *practice in the backward direction.*

Further irregularities occur in the recitation. We observed a marked tendency for the subject to look away from the printed material from time to time and to attempt a private recitation. The items actually reproduced in these recitations are of course not always arranged in the proper order. The subject's own incorrect order frequently contains pairs and groups of items which are arranged in the backward direction. Further opportunity for practice in the backward direction is afforded when the subject makes his final recitation for the experimenter; and in such cases it is quite impossible to keep the subject from thinking rather vividly about his own incorrect arrangement of the items.

When the unavoidable irregularities occur, the associations which are established in the order *5-4-3-2-1* are not the result of the formation of associations in the order *1-2-3-4-5*, but simply the result of *forward* learning in the order *5-4-3-2-1*. The irregular associations formed while learning the practice lists also have the definite advantage of recency when the subject passes on to the test lists a few minutes later.

The irregularities just referred to affect the *B*-lists considerably, since the items in each *B*-list are adjacent to each other and are printed on the same card, when compared with the previous arrangement of the same items in the practice lists. These irregularities, however, do not affect the *S*-lists, because each of the items in a single *S*-list was previously a part of a different practice list, and the subject held in his hand and learned only one practice list at a time. If we consider the arrangement of all of the items in the practice lists, then the items in each *S*-list were previously separated from each other by about 14, 17, 20, or 26 intervening items, and they were also arranged in the backward direction. Therefore, since the learning of the *B*-lists begins *above zero*, on account of the faulty control, and since the learning of the

S-list begins *at zero*, the results given above are prejudiced in favor of the theory of backward association, and will have to be discounted to some extent.[8]

This interpretation seems to harmonize with some general tendencies which may be noted in Tables V, VI, VII, and VIII. It would be expected from the preceding discussion that an increase in the overlearning of the practice lists would be accompanied by greater reinforcing effect in the B-lists. It can be observed that as one passes from Table VI to Table VIII, there is an increase in the superiority of the *F*-lists and the B-lists over the *S*-lists. It would also follow from our discussion that a relatively larger number of irregularities and fluctuations should occur in learning long lists than in learning short lists; and it may be noted that the reliability of the results in favor of backward association increases as one passes from the short lists of Table V to the long lists of Table VIII. It is more difficult to control the learning process with long lists, and they generally require relatively more learning for one correct recitation.

We have shown that the learning of rote material under ordinary laboratory conditions results in the formation of backward associations, and *the theory is therefore true as a practical proposition under these conditions*. However, the irregularities and fluctuations which occur make it very doubtful whether the theory will apply to all learning situations and to all types of material. The question remains as to whether it is possible to control the conditions of learning in such a way that no backward associations will be formed. From a more practical point of view, it is also very desirable to know whether learning the more common meaningful material of everyday life results in the formation of backward associations. An attempt is made to answer these questions in Part II of the present paper.

[8] It should be observed that the interpretation of the results which we have given also applies in larger measure to the results obtained by Ebbinghaus, *op. cit.*, pp. 90–117.

PART II. TESTS WITH LOGICAL MATERIAL

Introduction

The experiment here to be described was a natural outgrowth of the difficulties and irregularities which were noted in Part I. Although both experiments are concerned with the same problem, the type of material and the procedure used in the two cases are different. The practice lists in the first experiment were learned to the point of one correct recitation on two different occasions; the tests with the practice lists were made a few minutes later, and the remote associations had the definite advantage of recency. The logical material in the present experiment was greatly overlearned, and suitable tests were given to determine whether this additional practice had resulted in the formation of a greater number of backward associations. The logical material was also retained over a period of several days, and tests were given to determine whether the remote associations had been forgotten, and whether the effect produced was a facilitation or possibly even an inhibition of backward associations. In contrast with the rote material and the laboratory situation of the first experiment, the subjects in the present experiment were simply given a paragraph of logical prose material and were instructed to take it home and memorize it at their leisure. When the subject is left to his own devices in this way, the conditions of learning are fairly natural and are similar to the common learning situations of everyday life, and some of the criticisms of the laboratory experiments in learning nonsense syllables are avoided.

Material

The prose passage selected [9] is an adaptation from James' chapter on habit (*Principles of Psychology*, Vol. I, 127). It is quoted in full as follows.

[9] We were partly guided in the selection of the logical prose material by the factors of natural interest and literary style. The general appropriateness for the occasion was also considered, because at the time the experiment was carried out the subjects were taking the writer's summer course in elementary psychology, in which the topic of instinct had just been discussed.

TABLE XII

THE FORMATION OF 10 BACKWARD LISTS AND 10 SEPARATED LISTS FROM THE LOGICAL PROSE MATERIAL

MENTAL	CONDITIONS	IS	A	MOST	POWERFUL	ALLY	OF	ETHICS	THE	10*B*
HELL	*TO*	*BE*	*ENDURED*	*HEREAFTER*	*OF*	*WHICH*	*THEOLOGY*	*TELLS*	*IS*	
NO	WORSE	THAN	THE	HELL	WE	MAKE	FOR	OURSELVES	IN	9*B*
THIS	*WORLD*	*BY*	*HABITUALLY*	*FASHIONING*	*OUR*	*CHARACTERS*	*IN*	*THE*	*WRONG*	
WAY	COULD	THE	YOUNG	BUT	REALIZE	HOW	SOON	THEY	WILL	8*B*
BECOME	*MERE*	*WALKING*	*BUNDLES*	*OF*	*HABITS*	*THEY*	*WOULD*	*GIVE*	*MORE*	
HEED	TO	THEIR	CONDUCT	WHILE	IN	THE	PLASTIC	STATE	EVERY	7*B*
SMALLEST	*STROKE*	*OF*	*VIRTUE*	*OR*	*OF*	*VICE*	*LEAVES*	*ITS*	*NEVER*	
SO	LITTLE	SCAR	THE	DRUNKEN	RIP	VAN	WINKLE	IN	JEFFERSON'S	6*B*
PLAY	*EXCUSES*	*HIMSELF*	*FOR*	*EVERY*	*FRESH*	*DERELICTION*	*BY*	*SAYING*	*I*	
WON'T	COUNT	THIS	TIME	WELL	HE	MAY	NOT	COUNT	IT	5*B*
AND	*A*	*KIND*	*HEAVEN*	*MAY*	*NOT*	*COUNT*	*IT*	*BUT*	*IT*	
IS	BEING	COUNTED	NONE	THE	LESS	DOWN	AMONG	HIS	NERVE	4*B*
CELLS	*AND*	*FIBRES*	*THE*	*MOLECULES*	*ARE*	*COUNTING*	*IT*	*REGISTERING*	*AND*	
STORING	IT	UP	TO	BE	USED	AGAINST	HIM	WHEN	THE	3*B*
NEXT	*TEMPTATION*	*COMES*	*NOTHING*	*WE*	*EVER*	*DO*	*IS*	*IN*	*STRICT*	
SCIENTIFIC	LITERALNESS	WIPED	OUT	OF	COURSE	THIS	HAS	ITS	GOOD	2*B*
SIDE	*AS*	*WELL*	*AS*	*ITS*	*BAD*	*ONE*	*AS*	*WE*	*BECOME*	
PERMANENT	DRUNKARDS	BY	SO	MANY	SEPARATE	DRINKS	SO	WE	BECOME	1*B*
SAINTS	*IN*	*THE*	*MORAL*	*AND*	*AUTHORITIES*	*AND*	*EXPERTS*	*IN*	*THE*	
10*S*	9*S*	8*S*	7*S*	6*S*	5*S*	4*S*	3*S*	2*S*	1*S*	

THE STUDY OF MENTAL CONDITIONS IS A MOST POWERFUL ALLY OF ETHICS. THE HELL TO BE ENDURED HEREAFTER, OF WHICH THEOLOGY TELLS, IS NO WORSE THAN THE HELL WE MAKE FOR OURSELVES IN THIS WORLD BY HABITUALLY FASHIONING OUR CHARACTERS IN THE WRONG WAY. COULD THE YOUNG BUT REALIZE HOW SOON THEY WILL BECOME MERE WALKING BUNDLES OF HABITS, THEY WOULD GIVE MORE HEED TO THEIR CONDUCT WHILE IN THE PLASTIC STATE. EVERY SMALLEST STROKE OF VIRTUE OR OF VICE LEAVES ITS NEVER SO LITTLE SCAR. THE DRUNKEN RIP VAN WINKLE, IN JEFFERSON'S PLAY, EXCUSES HIMSELF FOR EVERY FRESH DERELICTION BY SAYING, 'I WON'T COUNT THIS TIME!' WELL! HE MAY NOT COUNT IT, AND A KIND HEAVEN MAY NOT COUNT IT; BUT IT IS BEING COUNTED NONE THE LESS. DOWN AMONG HIS NERVE CELLS AND FIBERS THE MOLECULES ARE COUNTING IT, REGISTERING AND STORING IT UP TO BE USED AGAINST HIM WHEN THE NEXT TEMPTATION COMES. NOTHING WE EVER DO IS, IN STRICT SCIENTIFIC LITERALNESS, WIPED OUT. OF COURSE, THIS HAS ITS GOOD SIDE AS WELL AS ITS BAD ONE. AS WE BECOME PERMANENT DRUNKARDS BY SO MANY SEPARATE DRINKS, SO WE BECOME SAINTS IN THE MORAL, AND AUTHORITIES AND EXPERTS IN THE PRACTICAL AND SCIENTIFIC SPHERES, BY SO MANY SEPARATE ACTS AND HOURS OF WORK.

Table XII shows the method which was used in deriving 10 backward lists and 10 separated lists from the prose selection. The first 3 and the last 13 words of the prose passage are omitted in the table, which contains 200 words arranged in the same order as in the prose selection. The first backward list consists of the 10 words in the row to the left of "1*B*" in the table, arranged and written in the reverse order, as "BECOME WE SO DRINKS SEPARATE MANY SO BY DRUNKARDS PERMANENT." Each of the remaining *B*-lists, 2*B*, 3*B*, etc., is constructed in the same way. The first separated list is made up of the 10 italicized words in the column above "1*S*" in the table, reading from bottom to top, as "THE BECOME STRICT AND IT I NEVER MORE WRONG IS," etc.

Procedure

The procedure which we have devised may be divided into 3 parts: (1) Learning the 20 *B*- and *S*-lists, (2) Learning the prose passage, and (3) Relearning the 20 *B*- and *S*-lists. It is possible in this way to compare the efficiency of learning the *B*- and *S*-lists, both before and after learning the prose passage.

Learning the B- and S-lists.—The writer acted as experimenter, and the subjects were taken individually. The subject first learned three preliminary practice lists, thereby becoming acquainted with the procedure. The method of learning was similar to that used with the test lists in Part I, except that the efficiency of learning was measured in all cases in terms of the time required to learn each list to the point of one correct and ready recitation.

The subject was then required to learn the 20 *B*- and *S*-lists singly, in the order 1*B*, 1*S*, 2*B*, 2*S*, etc. An effort was made to control the conditions in such a way that the subject should not know that the *B*-lists were made up of words taken from a logical passage and arranged in the reverse order. At the end of the one-hour period required for this procedure, the subject was questioned about the construction of the lists their relative difficulty, etc. No subject noticed the method of construction of any of the lists. The time measures obtained make it possible to compare the natural difficulty which each subject experienced in learning the *B*- and *S*-lists before learning the prose passage.

Learning the prose passage.—Each of the 31 subjects was taken for the one-hour period in the individual procedure described above between the Monday and Saturday of one week. When the class met on the following Monday, printed copies of the prose passage were distributed, and the group of subjects was instructed to take the passage home and learn it. On the Tuesday, Wednesday, Thursday, and Friday of this week, the first 10-minutes of the class-period were used in having the subjects recite the prose passage to each other in pairs. At the end of the second week, the subjects were urged to continue practicing the prose passage every day, so that they could recite it without error during the third and last week of the experiment.

It is well to point out that logical material has certain very definite advantages over rote material for the purposes of the present experiment. When the rote material (of Part I) is used, the subject's thought processes wander and fluctuate on account of the general lack of interest and meaning in the

disconnected items. It is probably not possible to control the thinking processes in practice with such material. But the case is somewhat different with the logical material. After the prose passage has been learned to the point of one correct recitation, further repetition results in a condition in which the process of overlearning is admirably controlled. The subject recognizes his own errors, and promptly makes the desired corrections himself. He is held in line in a form of practice which is more or less automatically controlled. As the controlled practice continues, the overlearning becomes more and more pronounced, and, as we shall see later, the weak remote associations formed by the irregularities in the early stages of the practice are practically forgotten.

Relearning the B- and S-lists.—Each subject was scheduled for a one-hour period exactly 2 weeks after the first learning of the *B*- and *S*-lists. He was first asked to recite the prose passage. No comments were made on his recitation, and his errors were not corrected. The subject then relearned the *B*- and *S*-lists, using the same method as before.

When the third and last part of the procedure had been completed, the subject was asked a number of questions which were designed to find out whether he had any insight into the problem being investigated. He was asked whether he noticed the method of construction of the 20 lists, how many of the words had been taken from the prose passage, whether the lists could be divided into several classes according to any criterion, why some lists were harder to learn than others, the reason for the soft spots and the difficult places, etc. Special care was taken not to suggest the method of constructing the 20 lists and the general purpose of the experiment. Most of the subjects thought that only about three-fourths of the words in the 20 lists had been taken from the prose passage. *No subject suspected the method of constructing the B- and S-lists from the prose passage.* The writer also asked the subjects for their opinion in regard to the general purpose of the experiment, and the reason for using this type of material. Perhaps the most significant feature of our procedure is that *no subject had any insight into the problem being investigated.*

Results

Influence of learning prose passage on backward association: individual results.—26 out of a total of 31 members of the class in elementary psychology completed the procedure as we have described it above. The individual results for these 26 subjects are given in Tables XIII*A–D*. The general method of treating the data may be illustrated by reference to the results of the first subject, shown in Table XIII*A*.

The first column in the Table (A) shows that this subject required an average of about 39 sec to learn the 10 *B*-lists, and an average of about 41 sec to learn the 10 *S*-lists. The superiority of the *B*-lists over the *S*-lists amounts to 2 sec. This difference is expressed in per cent. by dividing 2 by the average of 39 and 41.

After the learning of the prose passage, the same 10 *B*-lists required an average learning time of 17 sec, and the same 10 *S*-lists required an average learning time of 28 sec. The difference is now + 11 sec, in favor of the *B*-lists.

The superiority of the *B*-lists over the *S*-lists is greater after learning the prose passage than before. The difference $(d–c) - (b–a)$ is + 9 sec, or + 43 per cent. It appears from the results of this subject that learning the prose passage has resulted in the formation of associations in the backward direction.

The difference after learning $(d–c)$ is more significant than the difference before learning $(b–a)$ on account of the shift in the base of comparison. For most purposes, the difference $(d–c) - (b–a)$ is not as important as the percentage difference given in the lower row of the table. If the figures in the lower row are positive, the results are in favor of backward association; if these figures are negative the results indicate that the formation of associations in the backward direction has been inhibited.

Tables XIII*A–D* show a variety of facts in regard to the individual tendencies of the 26 subjects. The value for $b–a$ is positive in 18 cases out of 26; and the value for $d–c$ is positive in 14 cases of out 26. This indicates that the effect of learning the prose passage is to inhibit the formation of

TABLES XIII*A* AND XIII*B*

INFLUENCE OF LEARNING PROSE PASSAGE ON BACKWARD ASSOCIATION; INDIVIDUAL RESULTS

Subjects	1		2		3		4		5		6		7	
Central Tendency	Ave.	Med.	Ave.	Med.	Ave.	Med.	Ave.	Med.	Ave.	Med.	Ave.	Med.	Ave.	Med.
Before Learning Passage:														
(*a*) *B*-Lists	38.7	35.5	33.0	27.0	41.4	40.5	37.8	34.5	55.1	48.5	32.8	27.5	72.4	59.0
(*b*) *S*-Lists	40.8	38.0	28.7	27.5	44.4	41.5	41.9	41.5	57.5	58.5	45.3	50.5	73.2	70.0
b–*a*	+ 2.1	+ 2.5	− 4.3	+ 0.5	+ 3.0	+ 1.0	+ 4.1	+ 7.0	+ 2.4	+10.0	+12.5	+23.0	+ 0.8	+11.0
(Per cent.)	+ 5.3	+ 6.8	−13.9	+ 1.8	+ 7.0	+ 2.4	+10.3	+18.4	+ 4.3	+18.7	+32.0	+59.0	+ 1.1	+17.1
After Learning Passage:														
(*c*) *B*-Lists	17.2	16.0	14.0	11.0	22.7	20.0	16.4	13.5	21.9	18.0	19.3	16.5	60.7	53.0
(*d*) *S*-Lists	28.3	21.0	17.7	18.5	23.6	23.0	25.0	22.5	30.9	28.5	37.9	34.0	70.5	61.5
d–*c*	+11.1	+ 5.0	+ 3.7	+ 7.5	+ 0.9	+ 3.0	+ 8.6	+ 9.0	+ 9.0	+10.5	+18.6	+17.5	+ 9.8	+ 8.5
(Per cent.)	+48.8	+27.0	+23.3	+50.8	+ 3.9	+13.9	+41.5	+50.0	+34.1	+45.2	+65.0	+69.3	+14.9	+14.8
Effect of Learning Passage, or (*d*–*c*) − (*b*–*a*)	+ 9.0	+ 2.5	+ 8.0	+ 7.0	− 2.1	+ 2.0	+ 4.5	+ 2.0	+ 6.6	+ 0.5	+ 6.1	− 5.5	+ 9.0	− 2.5
(Per cent.)	+43.5	+20.2	+37.2	+49.0	− 3.1	+11.5	+31.2	+31.6	+29.8	+26.5	+33.0	+10.3	+13.8	− 2.3

Subjects	8		9		10		11		12		13	
Central Tendency	Ave.	Med.	Ave.	Med.	Ave.	Med.	Ave.	Med.	Ave.	Med.	Ave.	Med.
Before Learning Passage:												
(*a*) *B*-Lists	76.0	73.5	53.9	51.5	76.2	72.5	50.1	35.5	19.8	16.5	35.3	28.0
(*b*) *S*-Lists	84.3	90.0	56.6	61.0	57.2	52.5	58.5	63.0	23.7	22.0	47.3	46.5
b–*a*	+ 8.3	+16.5	+ 2.7	+ 9.5	−19.0	−20.0	+ 8.4	+27.5	+ 3.9	+ 5.5	+12.0	+18.5
(Per cent.)	+10.4	+20.2	+ 4.9	+16.9	−28.5	−32.0	+15.5	+55.8	+17.9	+28.6	+29.0	+49.7
After Learning Passage:												
(*c*) *B*-Lists	45.7	43.0	33.7	30.0	34.6	30.0	34.5	23.5	16.6	14.0	27.3	18.5
(*d*) *S*-Lists	51.4	52.0	28.2	29.0	35.0	31.0	25.5	24.5	13.5	13.5	23.8	23.0
d–*c*	+ 5.7	+ 9.0	− 5.5	− 1.0	+ 0.4	+ 1.0	− 9.0	+ 1.0	− 3.1	− 0.5	− 3.5	+ 4.5
(Per cent.)	+11.7	+18.9	−17.8	− 3.4	+ 1.1	+ 3.3	−30.0	+ 4.2	−20.6	− 3.6	−13.7	+21.7
Effect of Learning Passage, or (*d*–*c*) − (*b*–*a*)	− 2.6	− 7.5	− 8.2	−10.5	+19.4	+21.0	−17.4	−26.5	− 7.0	− 6.0	−15.5	−14.0
(Per cent.)	+ 1.3	− 1.3	−22.7	−20.3	+29.6	+35.3	−45.5	−51.6	−38.5	−32.2	−42.7	−28.0

TABLES XIIIC AND XIIID

INFLUENCE OF LEARNING PROSE PASSAGE ON BACKWARD ASSOCIATION; INDIVIDUAL RESULTS

Subjects	14		15		16		17		18		19		20	
Central Tendency	Ave.	Med.	Ave.	Med.	Ave.	Med.	Ave.	Med.	Ave.	Med.	Ave.	Med.	Ave.	Med.
Before Learning Passage:														
(*a*) *B*-Lists	69.6	57.0	73.1	62.0	40.9	38.5	33.4	31.0	35.1	30.5	32.8	27.0	50.6	42.5
(*b*) *S*-Lists	67.5	70.0	87.2	62.5	53.5	52.5	30.2	24.5	29.6	29.5	41.4	35.5	47.0	42.5
b–*a*	− 2.1	+13.0	+14.1	+ 0.5	+12.6	+14.0	− 3.2	− 6.5	− 5.5	− 1.0	+ 8.6	+ 8.5	− 3.6	0.0
(Per cent.)	− 3.1	+20.5	+17.6	+ 0.8	+26.7	+30.8	−10.1	−23.4	−17.0	− 3.3	+23.2	+27.2	− 7.4	0.0
After Learning Passage:														
(*c*) *B*-Lists	50.7	41.5	31.4	32.5	25.8	27.0	20.0	16.0	15.0	13.0	31.5	22.0	27.6	23.5
(*d*) *S*-Lists	43.3	39.0	32.7	28.5	25.9	23.5	14.3	11.0	14.4	13.5	22.4	23.0	24.0	23.5
d–*c*	− 7.4	− 2.5	+ 1.3	− 4.0	+ 0.1	− 3.5	− 5.7	− 5.0	− 0.6	+ 0.5	− 9.1	+ 1.0	− 3.6	0.0
(Per cent.)	−15.7	− 6.2	+ 4.1	−13.1	+ 0.4	−13.9	−33.2	−37.0	− 4.8	+ 3.8	−33.8	+ 4.4	−13.9	0.0
Effect of Learning Passage, or (*d*–*c*) − (*b*–*a*)	− 5.3	−15.5	−12.8	− 4.5	−12.5	−17.5	− 2.5	+ 1.5	+ 4.9	+ 1.5	−17.7	− 7.5	0.0	0.0
(Per cent.)	−12.6	−26.7	−13.5	−13.9	−26.3	−44.7	−23.1	−13.6	+12.2	+ 7.1	−57.0	−22.8	− 6.5	0.0

Subjects	21		22		23		24		25		26	
Central Tendency	Ave.	Med.	Ave.	Med.	Ave.	Med.	Ave.	Med.	Ave.	Med.	Ave.	Med.
Before Learning Passage:												
(*a*) *B*-Lists	63.8	63.0	83.7	75.5	26.1	19.0	43.7	35.5	38.1	33.0	15.4	13.5
(*b*) *S*-Lists	61.9	64.0	75.0	76.0	30.9	31.0	52.8	49.5	40.9	43.0	17.1	12.0
b–*a*	− 1.9	+ 1.0	− 8.7	+ 0.5	+ 4.8	+12.0	+ 9.1	+14.0	+ 2.8	+10.0	+ 1.7	− 1.5
(Per cent.)	− 3.0	+ 1.6	−11.1	+ 0.7	+16.8	+48.0	+18.9	+32.9	+ 7.1	+26.3	+10.5	−11.8
After Learning Passage:												
(*c*) *B*-Lists	19.7	23.0	40.3	34.5	15.2	12.5	24.0	26.0	20.2	20.5	13.0	8.5
(*d*) *S*-Lists	26.5	26.0	48.8	45.0	15.7	16.0	25.6	25.0	21.1	17.0	9.1	7.5
d–*c*	+ 6.8	+ 3.0	+ 8.5	+10.5	+ 0.5	+ 3.5	+ 1.6	− 1.0	+ 0.9	− 3.5	− 3.9	− 1.0
(Per cent.)	+29.4	+12.2	+19.1	+26.4	+ 3.2	+24.6	+ 6.5	− 3.9	+ 4.4	−18.7	−35.3	−12.5
Effect of Learning Passage, or (*d*–*c*) − (*b*–*a*)	+ 8.7	+ 2.0	+17.2	+10.0	− 4.3	− 8.5	− 7.5	−15.0	− 1.9	−13.5	− 5.6	+ 0.5
(Per cent.)	+32.4	+10.6	+30.2	+25.7	−13.6	−23.4	−12.4	−36.8	− 2.7	−45.0	−45.8	− 0.7

associations in the backward direction. The value of $(d-c) - (b-a)$ is positive in 11 cases and negative in 15 cases out of 26.

Influence of learning prose passage on backward association; combined results of 26 subjects.—All of the gross time scores for the 26 subjects are arbitrarily combined in Table XIV.

TABLE XIV

INFLUENCE OF LEARNING PROSE PASSAGE ON BACKWARD ASSOCIATION; COMBINED RESULTS OF 26 SUBJECTS

Statistical Measure	Ave.	Q_1	Q_2	Q_3
Before Learning Passage:				
(*a*) *B*-Lists	46.92±1.27	26.61	39.50±1.59	62.00
No. cases	260	260	260	260
S. D.	30.26			
(*b*) *S*-Lists	49.77±1.24	28.79	45.50±1.55	66.50
No. cases	260	260	260	260
S. D.	29.57			
b–a	+2.85±1.77	+2.18	+6.00±2.22	+4.50
(Per cent.)	+5.9	+7.9	+14.1	+7.0
After Learning Passage:				
(*c*) *B*-Lists	26.79±0.84	13.79	21.64±1.05	33.83
No. cases	260	260	260	260
S. D.	20.18			
(*d*) *S*-Lists	28.18±0.83	14.70	23.21±1.04	36.70
No. cases	260	260	260	260
S. D.	19.88			
d–c	+1.39±1.18	+0.91	+1.57±1.48	+2.87
(Per cent.)	+5.1	+6.4	+7.0	+8.1
Effect of Learning Passage, or $(d-c)-(b-a)$	−1.46±2.13	−1.27	−4.43±2.67	−1.63
(Per cent.)	−0.8	−1.5	−7.1	+1.1

The general method of presentation is already familiar. The results as a whole are opposed to the theory of backward associations, inasmuch as all of the values for $(d-c) - (b-a)$ are negative. The probable errors would have been much smaller if all of the subjects had shown approximately the same ability in learning the *B*- and *S*-lists.

Influence of partly learning prose passage on backward association; individual and combined results of 5 subjects.—It will be recalled that the plan of procedure required the subject to learn the prose passage before relearning the *B*- and *S*-lists. It so happened that 5 out of the 31 students did not carry out the instructions, and they only knew certain parts of the

TABLE XV

INFLUENCE OF PARTLY LEARNING PROSE PASSAGE ON BACKWARD ASSOCIATION; INDIVIDUAL RESULTS

Subjects	*A*		*B*		*C*		*D*		*E*	
Central Tendency	Ave.	Med.	Ave.	Med.	Ave.	Med.	Ave.	Med.	Ave.	Med.
Before Learning Passage:										
(*a*) *B*-Lists	86.6	83.5	49.4	46.0	158.9	132.0	82.9	77.0	87.4	58.0
(*b*) *S*-Lists	88.2	74.0	45.2	40.5	154.1	13.55	68.5	74.0	53.6	46.5
b–*a*	+ 1.6	− 9.5	− 4.2	− 5.5	− 4.8	+ 3.5	−14.4	− 3.0	−33.8	−11.5
(Per cent.)	+ 1.8	−12.1	− 8.0	−12.7	− 3.1	+ 2.6	−19.0	− 4.0	−47.9	−22.0
After Learning Passage:										
(*c*) *B*-Lists	26.1	26.5	31.8	26.5	63.3	61.5	40.6	35.5	34.1	31.5
(*d*) *S*-Lists	45.8	49.5	42.1	42.5	80.7	83.0	63.0	57.5	45.0	47.5
d–*c*	+19.7	+23.0	+10.3	+16.0	+17.4	+21.5	+22.4	+22.0	+10.9	+16.0
(Per cent.)	+54.8	+60.5	+27.9	+46.0	+24.2	+29.8	+43.2	+47.3	+27.6	+40.1
Effect of Learning Passage, or (*d*–*c*) − (*b*–*a*)	+18.1	+32.5	+14.5	+21.5	+22.2	+18.0	+36.8	+25.0	+44.7	+27.5
(Per cent.)	+53.0	+72.6	+36.8	+58.7	+27.3	+27.2	+62.2	+51.3	+75.5	+62.1

prose passage when called upon to recite. The individual results for these 5 subjects are given in Table XV, and their combined results are given in Table XVI.

TABLE XVI

INFLUENCE OF PARTLY LEARNING PROSE PASSAGE ON BACKWARD ASSOCIATION; COMBINED RESULTS OF 5 SUBJECTS

Statistical Measure	Ave.	Q_1	Q_2	Q_3
Before Learning Passage:				
(*a*) *B*-Lists	93.04±6.72	48.00	71.00± 8.40	119.00
No. cases	50	50	50	50
S. D.	70.48			
(*b*) *S*-Lists	81.92±5.13	46.00	70.00± 6.41	89.00
No. cases	50	50	50	50
S. D.	53.77			
b–*a*	−11.12±8.45	− 2.00	− 1.00±10.56	−30.00
(Per cent.)	−12.7	− 4.3	− 1.4	−28.8
After Learning Passage:				
(*c*) *B*-Lists	39.18±2.06	26.00	33.50± 2.58	45.00
No. cases	50	50	50	50
S. D.	21.61			
(*d*) *S*-Lists	55.32±2.85	31.00	51.00± 3.56	66.00
No. cases	50	50	50	50
S. D.	29.86			
d–*c*	+16.14±3.52	+ 5.00	+17.50± 4.40	+21.00
(Per cent.)	+34.2	+17.5	+41.4	+37.8
Effect of Learning Passage, or (*d*–*c*)–(*b*–*a*)	+27.26±9.15	+ 7.00	+18.50±11.44	+51.00
(Per cent.)	+46.9	+21.8	+42.8	+66.6

The tables show that the *B*-lists are more difficult than the *S*-lists before learning the prose selection, but that the *B*-lists are easier than the *S*-lists after learning the selection. It appears from the figures in Tables XV and XVI that, *when the learning of a prose passage is weak and halting, a large number of backward associations are formed.* We may recall that the results for the 26 subjects show that, *when the prose passage is well learned, there is on the whole an inhibition of backward associations. According to the theory of backward association, however, the strength of the backward associations should increase with an increase in forward learning. These results show very clearly that increasing the forward learning decreases the backward association.*

A similar comparison can be made between the results of the 26 subjects described above and the results obtained with the rote material in Part I. The rote material (practice lists)

was just barely learned, and 'backward associations' were apparently formed. The logical material in the present experiment was greatly overlearned, and there was on the whole an inhibition of backward associations. These facts are definitely *opposed* to the theory of backward association.

Interpretation of Results

Probably the most reliable conclusion that can be drawn from the results of the experiments described in Parts I and II is that certain factors influence the learning process in such a way that the chances are practically equal of obtaining evidence in favor of or opposed to the theory of backward association. Although the variability of the results is important from a practical point of view, this does not exclude the possibility that some *general principle* may be at work. We have already attempted to show that the tendency in favor of backward association, when rote material is used, is the result of an inability to control the learning process. We have still to explain the apparent inhibition of backward associations with the logical material.[10]

Let us suppose that the 200 words in Table XII are numbered consecutively. Then the 10th *B*-list, for example, will be made up of the items *10-9-8-7-6-5-4-3-2-1*; and the 10th *S*-list will be made up of the items *191-171-151-131-111-91-71-51-31-11*. Now when the material in the second part of the procedure is practiced and learned in the forward direction, an association is formed between each item and the item immediately following, *i.e.*, from *1* to *2*, from *2* to *3*, etc., throughout the series. These associative tendencies persist of course in all of the items of the *B*- and *S*-lists when these lists are relearned in the third part of the procedure. We may set down the items of the 10th *B*- and *S*-lists in large numerals, with the associative tendencies of the items in small numerals, as follows:

B:	10^{-11}	9^{-10}	8^{-9}	7^{-8}	6^{-7}	5^{-6}	4^{-5}	3^{-4}	2^{-3}	1^{-2}
S:	191^{-192}	171^{-172}	151^{-152}	131^{-132}	111^{-112}	91^{-92}	71^{-72}	51^{-52}	31^{-32}	11^{-12}

[10] *Cf.* G. Frings, Ueber den Einfluss der Komplexbildung auf die effektuelle und generative Hemmung, *Arch. f. d. ges. Psychol.*, 1914, 30, 415–479; F. Nagel, Experimentelle Untersuchungen über Grundfragen der Assoziationslehre, *Arch. f. d. ges. Psychol.*, 1912, 23, 156–253; E. Claparède, *L'association des idées*, 1903, 141–150.

The first syllable in the *B*-list (no. *10*), for example, has a tendency to arouse syllable no. *11*; the second syllable in the *S*-list (*171*) has a tendency to arouse syllable *172*, etc. These associative tendencies will interfere with the relearning of both the *B*- and *S*-lists; but the inhibition will be greater for the *B*-list.

In the 10th *S*-list, shown above, all of the associative tendencies (*192*, *172*, etc.) were aroused in next to the last list which the subject learned before learning this list, since the *B*- and *S*-lists were learned in the order 1*B*, 1*S*, 2*B*, 2*S*, etc. In the *B*-list, however, the associative tendencies immediately precede the syllables, so that when the subject reaches the second syllable (*9*), for example, he tends to go back to the first syllable (*10*); when he reaches the third syllable (*8*) he tends to return to the second (*9*), etc. In the *B*-list, the associative tendency (preceding syllable) is quite 'prominent' because it has just been seen and named. The recent arousal of syllable *10* facilitates the recall of this syllable when syllable *9* is reached. The presence of these associative tendencies in the *B*-list not only interferes with the practice of this list but also with its final reproduction. The inhibiting tendency to shift backwards in the series should theoretically be greater in a *B*-list than in any other arrangement of the items.

The associative tendencies in the *S*-list have not been aroused so recently; they are not so active or so prominent, and they do not tend to make the subject turn backwards in the series. When the subject reaches syllable *9* in the *B*-list, he tends to go back to *10*, because he has just looked at it and named it. But when he reaches *171* in the *S*-list, he does not tend to go back to *191*, but rather to *172*. Several minutes have elapsed since *172* was aroused, and the tendency from *171* to *172* is not as strong as the tendency from *9* to *10*. It appears therefore that the associative tendencies from learning the prose passage inhibit the later learning of the *B*-lists more than the *S*-lists. But since the *S*-lists represent a zero base of learning, or a base of learning slightly below zero, a conclusion against the theory of backward association is

justified. *The act of learning the series 1-2-3-4-5, instead of establishing backward associations, forms inhibiting associations which interfere with the later learning of the same items in the order 5-4-3-2-1.*

The negative results of our study of backward association harmonize with the well-established physiological law of forward conduction across the synapse. Our results, however, do not agree with the results of the few other investigators who have studied this problem, Ebbinghaus, Müller and Schumann, Wohlgemuth and others. We shall not make a detailed comparison of the various results which have been obtained, but limit our remarks to pointing out one error in interpretation which several writers have made in regard to learning "paired associates."

When responses *A* and *B* are connected, the *practice* is frequently from *A* to *B* and also from *B* to *A*. If such is the case, *A* will evoke *B*, and *B* will also evoke *A*. The two responses have been integrated.[11] Some would say that they have combined. It is important to note, however, that the *practice* has been in *two* directions, and *two* associations have been formed.[12] The conditions obtaining when paired associates are learned are not suitable for testing the theory of backward association.[13] Some writers have erroneously assumed that the use of a memory exposure apparatus adequately controls the subject's thinking processes. Nevertheless, the subject can, and frequently does, think about an item in a series, even though the item is not visible at the time.

Summary

In order to test the theory of backward association a procedure with forward, backward, and separated lists was devised, in Part I, and nonsense syllables and short familiar

[11] Müller and Schumann (*op. cit.*) claimed that, when two consecutive syllables form a part of the same rhythmical measure, the second has a slight tendency to evoke the first.

[12] *Cf.* E. Claparède, *L'association des idées*, 1903, 128–130.

[13] *Cf.* especially A. Wohlgemuth, On memory and the direction of associations, *Brit. J. Psychol.*, 1913, 5, 447–465; Ueber die Richtung der Assoziationen, *Ber. über d. V. Kongress f. exper. Psychol.*, 1912, 284–287.

words were used as learning material. It is apparently impossible to limit the learning processes to the formation of associations in the forward direction when rote material is used. The various fluctuations and irregularities which occur in the learning process support the belief that when a series is being learned to all appearances in the forward direction, some *practice* is also taking place in the backward direction. The objective results show that when an effort is made to control the forward learning of rote material in a laboratory situation, some backward associations are formed in about 2 cases out of 3. Although the theory is true as a practical proposition under these conditions, the results give no justification for the belief that it will apply to all learning situations and to all types of material.

In Part II the subjects learned a logical prose passage and tests were given on later occasions to determine whether backward associations were also formed. It was found that the learning of logical material, instead of establishing backward associations, actually forms inhibiting associations which interfere with the later learning of the same items in the reverse order. The conclusion against the theory of backward association is further supported by the fact that the conditions of learning logical material can be controlled very satisfactorily. *The results emphasize the specific nature of the learning process when associations are being formed between the items of a series.*

The results of the present experiment on *backward association* should be considered in connection with a later experiment on *remote forward association*, since the two experiments deal with different aspects of the problem of remote association. The general significance of the results for the psychology of learning will be discussed in the later paper.

APPARENT VISUAL MOVEMENT AND THE *GESTALT*

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I. Nine Observations which stand against Wertheimer's Cortical Theory

The *Gestalttheorie* originated in the field of perception. In the apprehension of visual motion, as Wertheimer and others have maintained, lies the secret of many of our major psychological problems. An adequate understanding of this perception furnishes, as they think, a key to the comprehension of a wide range of experiences. Now theories are easy to formulate but difficult to sustain. The final test of any scientific assumption is experimental verification. If the facts of extended observation fail at substantiation, a doctrine can have little worth. We wish to indicate certain phases of the *Gestalt* which seem to possess limited scientific value because of an apparent conflict of theory with the facts of observation. Since the doctrine owes both its origin and its fullest expression to the study of apparent visual movement, we contend that the clearest understanding of its weaknesses as well as its strength may be had through a brief survey of the facts there alleged. An individual never outlives his history; and as much may be said for a theory. When a doctrine will not hold for the facts wherein it took its beginnings, it is immediately open to suspicion on that score; and where a theory has been mis-stated scientific interest demands an exposition of its exact significance shorn of its inadequacies. The inadequacies in the *Gestalttheorie* are most apparent in the treatment of visual movement. Let us consider some of them.

I. *Simultaneous apprehension of movement in different directions over identical regions.*—Visual movement has been ex-

plained in terms of a temporally and spatially extended process occurring, under certain objective conditions, between successively aroused cerebral regions. The most important of these conditions is the temporal interval between successive presentations of physical objects during which the neurological substratum of the movement experience completes itself. This neural process, although involving peripheral as well as central factors, may have no experiential counterpart save movement. In the interval elapsing between the physical expositions of the objects there is, according to Wertheimer and Koffka, an actual shift in the cortical energy from a cerebral region first excited towards a second region, thus conditioning the apprehension of movement from the first object over to the second.

Now a very simple demonstration of the inadequacy of such an explanation can be made in the following way. The first member of a two-membered figure is presented as an

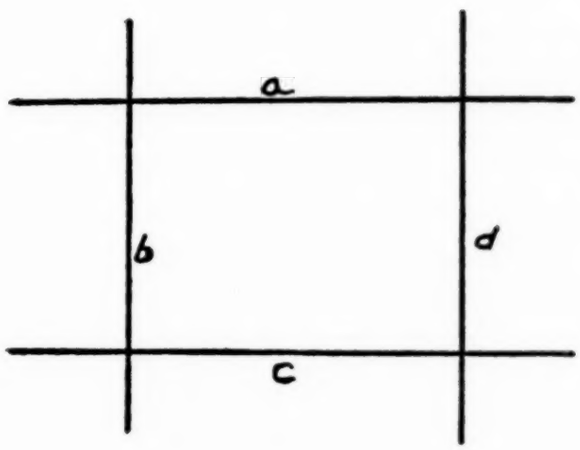


FIG. 1

upper horizontal line (*a* in Fig. 1) two inches in length with a vertical (*b*) intersecting its left end and extending a short way above and below. The second member is a similar horizontal line (*c*) with a vertical (*d*) intersecting its right end and extending above and below. The second member is so related to the first that, when presented with it, the entire Figure (1) appears. If both are exposed successively with about one inch between the horizontals, two very distinct and insistent

movements over the same part of the visual field at right angles to each other will be *simultaneously* apprehended. That is to say, one line (horizontal) moves clearly up and down while the other line (vertical) moves horizontally back and forth, over the same part of the field. Again, two simultaneous movements in *opposite* directions can be observed through the use of the following stimulus objects (Fig. 2). The first member is *a*; the second is *b*. Together they form *c*.

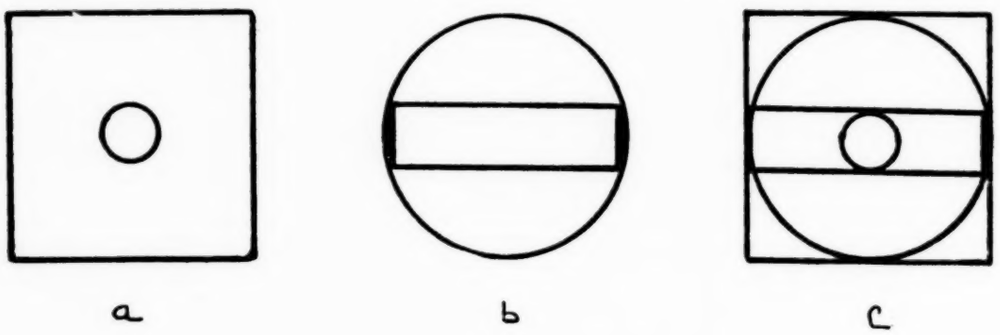


FIG. 2

Under successive presentations the larger circle shrinks to the size of the smaller and, at the same time, the smaller parallelogram moves out to the position of the larger. In this way are apprehended two movements over the same part of the visual field at the same moment. This experience cannot be explained by claiming a difference in *Gestalten; viz.*, that the circles form one set of figures and the rectangles another set. The clearest kind of movement may be observed either in the curious expansions and contractions of a circle to form a square or triangle, or in the movements of triangles to form circles or ovals. In the attempt to substantiate the physiological assumptions underlying the *Gestalttheorie* a large number of objects have been used to produce visual movements of various kinds. Wertheimer pointed out that movement in different directions may occur simultaneously over *different* parts of the visual field, while Korte showed that movement might, under certain conditions, be reversed. But at no place do we find mention of movement under the precise conditions (the kind of figures) described above. The apprehension of *simultaneous* apparent visual movement in

different directions over the same region seems to demonstrate the impossibility of attempting to bring generally the facts of motion under the functional neurology of the *Gestalt* as stated by Wertheimer, Koffka and Koehler; for the assumption of a short-circuiting or of a retinal fusion or of a mere difference in potential cannot hold under these conditions unless we believe in a two-way process occurring simultaneously within a given membrane with each part *wholly* independent of the other. Nothing comparable to the above appears to be known in electrical physics.

II. *Optimal movement under physical sequence but without a temporal interval.*—A further weakness of the explanation concerns this same matter of cortical function. Since the configurationist assumes a neural function characterized primarily by its temporal aspect, optimal movement can occur only when the physical conditions, *viz.*, length of exposition, length of pause, intensity of the stimulus-object, and the distance between successive members are harmoniously interrelated. But optimal movement occurs so clearly under conditions in which there is physical sequence without a temporal gap that such an explanation as the above becomes meaningless. We used two lights; one weak, the other strong.[1] The intensity of the stronger was sufficient to wipe out the figure obtained through the weaker, although the latter still remained lighted. Thus the removal of one figure meant the simultaneous appearance of the other because one could not appear until the other was gone; but when the one went the other had to appear. Under such conditions the clearest kind of movement was observable. The period of exposition seemed to be wholly unimportant for visual movement under these conditions. One member can be presented for a half-hour or more before the other is brought into the field without effecting the apprehension of movement seen when the second member finally comes to wipe out the first. Or the first may be presented for only a moment before the second object appears with just as clear movement of an optimal character. Koffka seems to believe that the period

[1] G. D. Higginson, *Amer. J. of Psychol.*, 1926, 37, 63–116.

during which the receptive surface of the eye is actually stimulated serves as a major condition of visual movement, but our results do not bear out such an assumption.

III. *Optimal movement under slight temporal delays* (*no physical sequence*).—The apprehension of movement obtained under the addition of one member to the other without temporal interval (just a delay) likewise fails to lend itself to an explanation in terms of the central theory. One member does not come and go to be followed by another; but one is given and then after a moment the other is added. Aside from the objection concerning the temporal interval necessary for the *phi-function* to run its course, we see in the movement secured under these conditions that the first object does not move; it is the second or the one added which carries the movement. The first may actually remain in its position while the second appears to disengage itself from it and move to its own final position. The first member may split into two parts; each forming a final member of the total figure. The phenomenal movement appears to be largely unaffected by the relative intensity of the two members. The first may be very weak or very strong as compared with the second; or it may be equal. The result is the same.

IV. *Optimal movement under removal of a part of the whole.*—Neither does the doctrine hold for movement obtained under subtraction of one member from another. The two members may be clearly joined by a phenomenal filling, by a light which extends out from and surrounds each member. When the light (one member) is turned out there should be merely a shrinkage of the member within itself. But one member is seen to disappear by way of the other. The visual perception of movement under subtraction and addition of members, as we view the matter, stands as a challenge to the adequacy of the neurological hypotheses of the doctrine embodied in the *Gestalt*. Under subtraction the course of the fusion must be just the reverse of that where the members are added. In the case of the former the supposed difference in potential is from the weak to the strong; in addition, it may be from equality to equality, or from strong to weak or from

weak to strong. Under these conditions the movement appears to retain its unity and its clearness.

V. *Partial movement.*—When we turn to the other forms of visual movement we again find it difficult to harmonize observation with theory. Certain outstanding features about partial movement—movement of a single member over a part of the interlinear field—cannot be forced under the rubrics of the configurational theory. Very frequently the first member is observed to be moving when it appears in the phenomenal field. It does not come into the field as a line and then move; it *moves* into the field. Furthermore, this

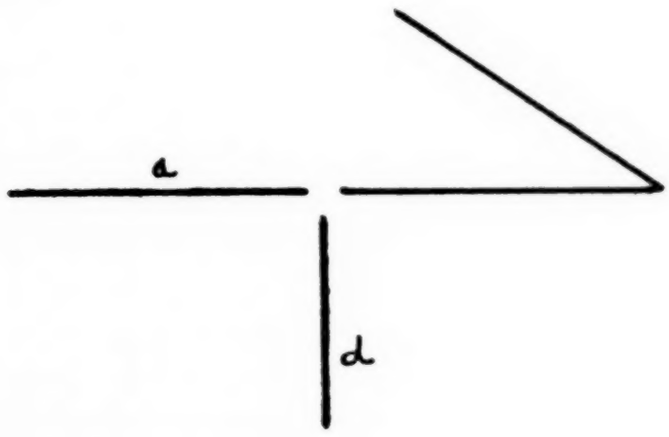


FIG. 3

movement may momentarily be in a direction opposed to the subsequent position of the second. Always, under these conditions, it is quite limited and immediately reverses and ends in the direction of the second member. At times, the partial movement may be of the second as it moves into position over a part of the field. After movement it stands out clearly as a line or figure. This sort of movement is just the reverse of that in which the first member is clearly seen and then disappears into blackness by *moving* over a part of the field between the two members. A striking demonstration of 'partial' movement appears where a second line (*a*) is placed at the end of the horizontal member of the oblique-horizontal arrangement so as to extend the horizontal outward two inches or more to the left beyond the end of the

oblique (Fig. 3). This extension need not even be *continuous* with the horizontal member. Under serial exposition, this line (prolonged horizontal) moves up and down, as one observer described it, 'like a whip.' In this case there is no other member directly *above* or *below* the line to condition in the organism any fusion or short-circuiting beneath the experiential level. There is, of course, the oblique which appears off to one side; but the movement is not in the direction of the oblique. It is up-and-down. This stimulus arrangement can be still further extended by dropping a vertical (*d*) from a place between the ends of the two contiguous horizontal lines. Then it is observed that this vertical line moves up and down synchronously with the movement of the horizontal proper and of the long horizontal extension. Here again is partial movement but without any possibility of a fusion such as is laid down, for example, by Koehler.

One other illustration must be cited in connection with partial movement. How shall we explain in terms of the doctrine that form of movement in which one member actually moves beyond the position of the other and then returns to it? This phenomenon has been repeatedly observed. Here the fusion must occur not only between the cortical regions, but it must, in some way, so reach beyond the boundaries of the second that the first is actually pulled over and beyond its legitimate goal which is the position of the second. The interpretation might be that the first pushed the second away from a position to which, after an instant, it returned.

VI. *Dual movement.*—If the doctrine finds itself partially inadequate in explaining full and partial movements, it encounters even more serious difficulties in explaining dual movements. The commonest form of dual movement in case of the oblique-horizontal arrangement is the clearly defined shift of both lines up and down or down and up, as though they were one rigid piece with the apex hinged to the background. The direction of the shift usually corresponds to the appearance and disappearance of the stimulus members. Where the oblique precedes the horizontal, the first shift of both lines is in the direction of the horizontal. Where the

order is reversed, the first movement is towards the oblique. The shift generally occurs in both directions even though each line is exposed but once. The lines may shift slightly (5–10°) in one direction and then again in the reverse direction, followed finally (in some instances) by a last movement in the first direction. In all cases there was a physical difference in time of at least 15 sigma. This slight difference might be said to condition the movement in the direction of the sequence of the lines. But what produces the other movements which may immediately follow? Physically regarded, the first object disappears a fraction of a second earlier. As phenomenally experienced, there is a change in the visual field. In terms of the *Gestalt* would there be the difference in potential—say, a leap—out to a place where there was nothing; that is, out to where the first object had been? The assumption of a short-circuiting cannot cover the facts of dual movement. One line moves but appears to drive or pull the other with it. Each keeps step with the other. The two shifts are wholly discrete. The movement is always of two lines which appear almost or wholly simultaneously in the field of vision. At other times, when the lines are not quite simultaneous, the report frequently runs that one member moves slightly ahead; just as a billiard ball moves across and assumes the position of another after driving it ahead. How the lines at one time move back and forth wholly independently of each other, and again at another time one moves, overtaking the other or driving it ahead, must be explained. In one case we have alternate 'repulsion' and 'attraction' apparently equal; in the other, strong attraction of the second line for the first followed by a weak repulsion.

VII. *Tri-dimensional movement.*—A survey of the experimental studies upon movement by members of the school of the *Gestalt* does not offer us an explanation of the tri-dimensional movements which may suddenly be apprehended under exactly the *same* objective conditions which previously have given two-dimensional movement. The previous studies have regarded motion in two dimensions. Such movement fits more easily the assumptions laid down by the doctrine. The

retina, so far as stimulation is concerned, is bi-dimensional; and while it is acted upon by spatially disjoined stimuli, apparently it does not differentiate between near and far objects. Retinal fusions or short-circuitings might occur, let us say, between certain stimulated points; but these points must, of necessity, lie on the surface. No one has argued that because one object lies nearer the retina than another it stimulates the retina in a manner spatially unlike another and more distant object. Even were this true, the question still remains as to how two figures which are equally distant from the retina and which give clear movement in *two* dimensions may suddenly be made to produce movement in a third di-

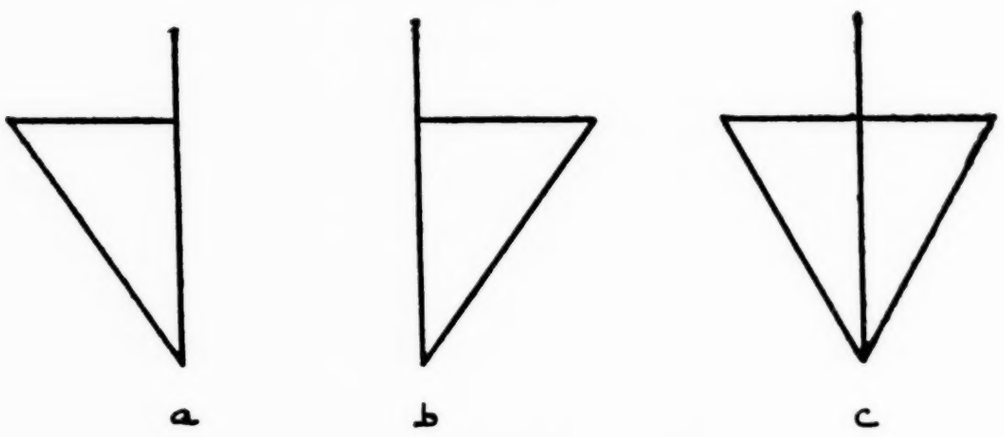


FIG. 4

mension. How in terms of the *Gestalttheorie* shall we explain the following? The first of a two-membered object is given as *a* (in Fig. 4); the second, *b* is similar in form and so related to the first that when the two are *together* they form *c*. Under successive expositions the oblique sides may rock alternately back and forth to the vertical as if they were hinged at the bottom. But a wholly different sort of movement may also be observed. One actually sees a triangle swinging around toward the observer upon the vertical as upon an axis. The same kind of movement may be readily observed under a variety of objective conditions. As one approaches brightness-equality with the ordinary Hess-Ives tintometer a clearly defined movement in the third dimension may be seen. The square

on the left (or right) turns over to the opposite side as the leaves of a book are turned.[2] Tri-dimensional movement seems to depend not so much upon the objective conditions as upon the character of the instruction in connection with the use of certain ambiguous figures. We must now consider this matter of instruction and its relation to visual movement.

VIII. *The effect of instruction upon the direction of movement.*—Through an apparent desire to establish abiding and causal relationships between the physical, physiological and phenomenal worlds the advocates of the *Gestalttheorie* have obviously underestimated the significance of the instruction and what it physiologically implies. As we find it, the effect of the instruction is so significant that it may result in the total character of the movement being changed. Thus we find that the direction of the movement may *instantly* change under a change in instruction. By using as the first member an upright cross and as the second member a similar cross so turned that its four arms bisect the four right angles of the first cross we actually obtain under serial exposition a rotary movement in either direction merely by changing the instruction. O could even make the star spin slowly around half way and then go back a third or all of the way; and then again resume the original direction. Or the use of the dots . · followed by this · . , so that together they formed a square : : gives movement in four directions depending solely upon the instruction. There may be an up-and-down movement, or a right-to-left movement or two movements in diagonal directions.

Now we cannot quite see how the configurationist can explain in terms of fusion, or in differences of potential, these sudden changes in the direction of movement under the instruction to observe such a change. Why should the star spin slowly clockwise or anti-clockwise under the instruction to see movement in these directions? Why should the dots move to the left and right instead of up and down; or from the lower-left to the upper-right without any change in the

[2] Professor Paul T. Young has called my attention to this movement with the tintometer.

physical conditions but with merely a change in instruction? The facts point to functions within the nervous system much more complex and intricate than the simple assumptions laid down in the doctrine against which we contend. Brain physiology, even in the perception of motion, does not appear to be reducible to short-circuitings or fusions unless the nature of these occurrences is quite different from that which is outlined in the theory under discussion. The facts, then, of instruction should make us critical concerning any theory which tends to neglect this very important mode of priming or of tuning the organism.

IX. *The presence of accessory members.*—Finally, we find the doctrine apparently unable to account for certain phenomena which it may be argued are not necessary for the apprehension of visual movement but which must be considered and explained because of their frequent occurrence and because they clearly indicate certain functional pecularities within the organism. We refer to the existence in the visual field of various phenomenal objects which could not possibly have been physically present. We find many secondary or intermediate lines accompanying the experience of visual movement. Os report the presence of as many as five lines where only two could have been. Dimmick's study likewise gives indication of these secondary members.[3] Their existence furnishes indubitable proof that the organism is functioning in a way that does not allow of explanation in terms of the *Gestalttheorie*. This doctrine attempts to explain the way in which the members which are physically given are bound together, but it cannot 'make' additional or *new* members. It remains then to be explained how so many of these curious secondary figures creep in where there should be only two. When the presence of these, as well as certain other phenomena, is explained, considerable progress will have been made towards an understanding of apparent visual movement.

[3] F. L. Dimmick, *Amer. J. of Psychol.*, 1920, 31, 317.

Conclusion

A survey of many facts observed in conjuction with the study of the perception of visual motion under discrete successive retinal stimulation indicates various weaknesses in the explanation of such movement as offered by the *Gestalttheorie*. In order to be adequate the doctrine must provide, we contend, for the following phenomena: (1) simultaneous movements in opposed directions over identical parts of the visual field, (2) visual movement where one member is presented and to it, after a slight delay, another is added, (3) visual movement where one member is removed from the total figure, (4) visual movement under physical sequence but without temporal interval, (5) partial movement where the other object is at rest or before the second object appears, (6) dual movements of various sorts, (7) tri-dimensional movement under precisely the same conditions which give bi-dimensional movement, (8) the significant changes induced in the direction of movement due merely to the change in instruction, and (9) the presence of various intermediate and accessory members. In terms neither of short-circuiting nor of cortical fusions nor of differences in potential, as these are now understood, can the above facts be explained. The resolution of the conflict between observation and theory calls either for a theoretical reinterpretation or for the abandonment of the specific neurological hypotheses underlying the whole of the explanation of visual movement as offered by theorists of the *Gestalt*.

II. The Effect upon Visual Movement of Colored Stimulus Objects

In his study *Ueber das Sehen von Bewegung* Wertheimer contends that the perception of visual movement may be independent, in a sense, of the quality of the stimulus. Red or white lines drawn upon a dark ground and successively exposed in a Schumann tachistoscope with a certain temporal interval between the objects gave rise, as he reported, to a visual perception of movement from the first to the second

object—with no object or color appearing intermediately—just as readily as did black lines upon a gray ground. The movement was not of a colored object; it was just movement.[1]

This apprehendion of 'colorless' movement with colored stimulus objects is very important for the general doctrine of the *Gestalt*. It may be cited as especially confirmatory of the independent and unitary character of the movement experience. Visual motion *per se* (*i.e.*, pure phi) has for the *Gestalttheorie* no qualitative moments. Since such an experience depends physiologically upon a dynamic *Struktur*, in no sense identical with the perception of the objects in the phenomenal field, it must, by its very nature, be independent of the quality of the stimulus.

More recently Dimmick, employing red, green and blue lines drawn upon white ground, red objects on green ground, and blue lines on yellow ground, reports that the apprehension of visual movement conditioned through such successively presented objects was resolvable into a 'grey flash' of brief duration. "Under the descriptive attitude there was always noted a *grey* patch covering the space between the two members." [2] The phenomenal field of movement then was uncolored; it was filled merely with a gray, presumably cortical in origin. This in spite of the fact that the stimulus objects as well as the background were colored. So Dimmick confirmed, in a manner, Wertheimer's results with respect to the *independent* character of visual movement under colored stimuli even though he rejected the physiological assumptions and the qualitative descriptions of the latter. From the results of an earlier study we were led seriously to question the primary importance for the visual apprehension of motion of any intermediate grey. We had such a large number of reports in which there was clear movement but no accompanying grey, such as Dimmick described, that we were forced

[1] Referring to the character of visual movement with colored stimuli Wertheimer writes, "Sehr deutliche, einheitliche Drehung sinnlich klar zu beschreibung . . . Ganzdrehung klar von *a* nach *b* gesehen, von der Mitte ist bezüglich des sonstigen Optischen zu sagen: da war nichts von Streifen, nichts von Rot." *Zsch. f. Psychol.*, 1912, 61, 222.

[2] F. L. Dimmick, *Amer. J. of Psychol.*, 1920, 31, 330.

to believe that its significance had been greatly exaggerated. Frequently our Os reported that the field was partially or wholly filled with a distinctly *yellowish* light either of a dynamic character—stretching and shooting; or of a more static nature. This filling apparently served to reënforce the impression of movement, since it was referred to as the 'cloud or haze of dust' similar to that raised by a natural object in motion. It could be perceived without movement or movement could be perceived without it, as a brief survey of the reports from our earlier study will show.[3] We felt that additional and valuable information upon the nature of visual movement might appear where the objective conditions were varied by way of a change in the quality of the stimulus objects. By using similarly and dissimilarly colored forms O was provided with a means of discriminating more sharply between the phenomenal objects. At the same time the task of defining the qualitative moments of the phenomenal field of movement would be simplified.

Problem.—We sought then to determine whether the visual perception of motion was qualitatively independent of the qualitative aspects of the stimulus objects or whether visual movement varied in quality with a change in the color of the stimulating conditions. That is to say, we sought to determine the *hue* of the object perceived as moving when the stimulus conditions were variously colored. If movement or the object-perceived-as-moving varies qualitatively from moment to moment with a change in the hue of the stimulus, we may assume that visual movement between two objects is *not* independent of the perceived figures but that it arises through the immediate integration of the successively apprehended object or objects. We can further assert that, if green lines give clear movement over a greenish field, and red lines give movement over a reddish field, the "existential correlate" of the apprehension of visual motion cannot possibly be said to be a "grey flash" fairly brief in duration.

Apparatus and method.—We used the apparatus previously constructed for a study of visual movement.[4] To obtain

[3] *Amer. J. of Psychol.*, 1926, 37, 63–116.

[4] *Op. cit.*, p. 76.

colored objects we placed in certain of the series (1–16) red and green gelatins over the windows of the tachistoscope as well as over the slitted cards. When the lines were similarly colored we also placed like-colored gelatins across the slits in the box before the eyes of O. Thus O saw two colored lines within or upon a black film or field. In the other series (17–32) we used colored figures drawn upon a colored and colorless ground. The successive objects were separated temporally by three intervals, 0, 75 sigma and 150 sigma. The formal instructions were the same as in the previous study. The Os were Dr. P. T. Young (Y) and Dr. E. A. Culler (C).

We presented the angular-horizontal and the parallel-horizontal arrangements of the lines in 32 series as follows:

I. *Objects upon lightless field. Temporal interval: 75 sigma and 150 sigma. Exposure time: 60 sigma.*

Single exposition of each figure

Angular arrangement

Ser. 1. Red to red
2. Red to green
3. Green to red
4. Green to green

Horizontal arrangement

Ser. 5. Red to red
6. Red to green
7. Green to red
8. Green to green

Serial exposition; each figure given five times

Angular arrangement

Ser. 9. Red to red
10. Red to green
11. Green to red
12. Green to green

Horizontal arrangement

Ser. 13. Red to red
14. Red to green
15. Green to red
16. Green to green

II. *Objects upon colored and colorless fields. No temporal interval. Exposure time: 5 sec.*

Single exposition of each figure
Angular arrangement
Ser. 17–20; same as Ser. 1–4
Horizontal arrangement
Ser. 21–24; same as Ser. 5–8
Serial expositions; each figure given five times
Angular arrangement
Ser. 25–28; same as Ser 9–12
Horizontal arrangement
Ser. 29–32; same as Ser. 13–16

Each of the first 16 series was presented to O with two intervals, 75 sigma and 150 sigma. In the method of serial exposition (9–16) the two lines were given five times. The 16 series were then repeated (series 17–32) without interval. The slitted cards were removed at *P* and black and gray cards with red and green lines, red cards with green lines, and yellow cards with red lines, were placed at *H*. One object was given for five sec by turning on the weaker of the two lights within the box, and then the other (stronger) was given to wipe out the first. Both were then removed together. Again, both lines were given simultaneously, although only one object—the brighter—was observable when the stronger light was on, and then the stronger was removed (after 5 sec) leaving the weaker. Under serial exposition the stronger was turned on and off at periods of 1 second.

Results; Series 1–16.—Since we are not primarily interested in the spatial extent of the apparent motion under these conditions we shall deal but briefly with that aspect of the experience. Where the lines were similarly colored optimal movement was generally reported. When the members were of dissimilar hue, there was a tendency to report fewer full movements. With unlike hues O reported at times that a line either moved partly across the field; or that it disappeared in motion and reappeared a moment later to complete the movement as a line of a different hue. But even with unlike

members the movement experience might be highly integrated. It meant that a single line travelled uniformly across the field changing in quality during movement. The fact that dissimilarly colored objects appear to incline the observer toward the apprehension of movement of the partial or dual type is important. If the perception of motion may be said to rest partly upon the successive apprehension of the same object through two moments of time and space it seems likely that where the successive phases are qualitatively different the continuity of the movement might easily be interrupted. Where successive lines of unlike hue are not apprehended as the same object O is led to report fewer cases of full and more cases of dual and partial movements. Much depends, it appears, upon the particular attitude or set of the observer. On the whole, we may say that where the objects are of similar hue, apparent motion tends more towards the optimal type. Qualitative peculiarities of the stimulus objects actually serve then to modify the extent of perceived motion, a mere change in hue being sufficient to change at times the character of the movement apprehended under discrete retinal stimulation.

What can we say of the qualitative aspects of the movement, *i.e.*, of the hue of the movement? O reported that, where movement was seen, the meaning was that a colored object travelled part or all the way across the field. Movement in every case was of a colored object, the particular hue depending upon the hue of the stimulus. That is to say, O reported that where there was any *relevant* visual filling for the movement it was colored red or green. When we say that movement was of a colored object, we mean that O saw such an object move, although there might actually be no discernible visual material in parts of the field. We indicated in an earlier study [5] that O may really apprehend movement of the clearest sort without being able to report the presence of anything other than the two lines or areal positions. O repeatedly claims that movement is continuous, uniform and clear; yet in the next breath remarks that there is nothing of a visual character filling the *whole* region between the lines to

[5] *Op. cit.*, 104 f.

carry the movement. The experience is so striking that even Os who have observed again and again will offer such comments as 'strange,' 'curious,' 'odd.' They unquestionably apprehend movement although a relevant visual filling is wanting. We cite a few reports from this series (1–16) upon the hue of the movement.

> I saw only two extreme positions: an upper red oblique and a lower red horizontal. Between them there was only blackness. I definitely saw the oblique move continuously to the horizontal position (C, 75).
>
> A red oblique moved to a horizontal position, paused for a moment and disappeared. I am sure of movement; it was quite clear, but I do not believe that there was any visual content (other than blackness) between the two positions (Y, 75).
>
> Two green lines forming an angle; around the upper and lower thirds of the sector the field was green. Between these areas the field was black. I saw a green oblique move to a horizontal green line. Movement was uniform and continuous although the visual apprehension of color was not (C, 150).
>
> A red horizontal line appeared; then a lower line similar to it. The field between was distinctly blackish, except for a colored haze around each line. A red horizontal line moved from upper to lower position (Y, 150).
>
> I saw a green horizontal line move quickly to a lower position. When it stopped it was red. The middle of the field was quite black (Y, 75).

At times, O apprehended a single line as moving across the field changing in hue during the course of the experience. It was as if the visual field were covered with two adjacent pieces of glass unlike in hue. The object moving, as if behind this glass, was now of one color, now of the other. Or, as O sometimes said, it was as though an object 'moved from the sunshine into the shade.' O found it difficult to point out the exact place of qualitative transition. One second the hue was red, the next it was green. In general it appears that the change was effected nearer the second member than the first; although it might actually occur in the middle of the region of movement. It seemed that the hue of the first line was held over until the second line came, so that everything up to the time of the appearance of the second tended to partake of the hue of the first. Where the lines were dissimilar in quality the movement was no less colored that where the lines were alike. We cite the following as representative of the reports upon the qualitative aspects of the visual movement with objects of like and different hues upon a lightless

ground. In some of them the nature of the change in hue, as O observed it, is indicated.

A green oblique appeared surrounded by a greenish yellow haze. Then a red horizontal came in accompanied by a reddish glow. I saw the green line move down about ¾ of the way toward the red stationary horizontal (Y, 150).

A red line moved continuously from an upper oblique to a lower horizontal position. About ⅓ of the way down the oblique was clearly defined for a moment. It was, however, less saturated (C, 150).

A green line moved from an oblique to a horizontal position (Y, 150).

A well-defined red horizontal line appeared; then lower down I saw three vaguely outlined red lines of low chroma appear on a reddish field. At the end, a fourth line much like the first. The whole experience meant that a red line distinctly moved from an upper to a lower horizontal position (Y, 75).

Movement of a single line from an upper to a lower horizontal position. During the first half, the movement was red; but over the second half it was green (C, 150).

A greenish oblique appeared followed by a red horizontal. The field was almost black with just a trace of green extending over the sector except the lower part, where the field was reddish. I saw a green line move from an oblique to a horizontal position and just as it reached the horizontal it turned red (Y, 75).

(Serial exposition; red and green): Two parallel bars, the upper red and the lower green, moving discretely up and down. Each is independent of the other; but both are related in movement. When the red line moves down, the green shifts with it. When the green moves up, the red precedes it. The lines keep step during movement (C, 75).

(Serial): A single line changing in color moved clearly and uniformly up and down over a sector. It was green going down but suddenly turned red upon reaching the bottom. Going up, the line was red until it reached the top where it suddenly changed to a greenish hue and started down again (Y, 150).

One of our major problems was to determine the phenomenal nature of the field intermediate to the two positions, *i.e.*, the qualitative aspects of the area over which the motion occurred. This region might be either partially or wholly colored. When partially colored the remainder was lightless or gray. Aside from the blacks and the greys, the visual filling, reds and greens, of various degrees of saturation and yellowishness, was referred to as being ill-defined, cloudlike, hazy, foggy or misty. The colors were blurry, sketchy, streaky or flashy. They either spread or shot out over the field much on the order of spilled paint, or else they were just there. These qualities were so obvious and so insistent that no one could possibly doubt their presence. Reports of such a positive character as these lead us to refer again to the at-

tempt to reduce visual movement to an existential "grey." To resort to such an explanation in the presence of the facts at hand not only does violence in the worst possible manner to the facts of observation but also obscures the true status of the movement experience. We can say that upon three occasions *only* in these series did O use the descriptive term grey. Then the report ran that the area around the end-positions was colored but that the *middle* (1/3 or less) of the sector was greyish. We cite, as illustrative of the general nature of the observations, parts of a few reports upon the filling intermediate to the two members.

A green line appeared in an oblique position and spread quickly downward to a red horizontal. This spreading was of an ill-defined yellowish-green cloud-like film (Y, 150).

A red oblique; beneath it a reddish cloud-like content; then a reddish horizontal. The whole sector was red (Y, 75).

The red oblique and the green horizontal areas are sharply separated by a small black sector. The red extends downward about ¼ inch and the green up a like distance (C, 150).

A red oblique and a horizontal green. A reddish-yellow cloud between (Y, 75).

The upper was a red; the lower, a green horizontal. A reddish haze extended down over ⅓ of the field, and a greenish haze covered the lower ⅓. Between these the field was quite black (C, 150).

An oblique red line; then a low chroma orangish film around and beneath it and extending down to a reddish horizontal line (Y, 75).

A green oblique appeared and spread downward over ¼ or ⅓ of the sector. Then the field was totally dark for a short distance and then a green line appeared and spread downward about ten degrees to form a horizontal (C, 150).

An angle formed by two green lines; the whole angle was filled with a greenish light (C, 75).

Two red lines forming an angle with a cloud-like blur, distinctly reddish in hue, between them (Y, 75).

A thin green horizontal appeared in the upper part of the visual field; then another similar line at the bottom. Between them the field was filled with a greenish haze (C, 75).

In addition to the vaguely defined colors intermediate to the end-positions Os reported the presence of secondary members or lines in a third (34 per cent.) of the observations. We have elsewhere indicated the theoretical significance of such 'accessory' objects, which have been observed in various major studies upon apparent visual movement of this type. They appear to contribute directly to the clear perception of

movement; for they are apparently seized upon and utilized by the organism as a resource in apprehending motion. They mean that a line is seen progressing across the field occupying these various positions. The chroma of these intermediates varied from time to time from a degree of saturation almost equal to that of the end lines to one of very little hue. In all cases but one, these lines were reported to be at least slightly colored. Where the two lines were complementary in hue, C reported once that at the point where the red and green films met, a colorless line was momentarily visible.

An interesting fact in connection with this matter of the secondary lines must be made clear. We believe that the particular hue of the intermediates when the end-positions were dissimilarly colored throws light upon certain functional operations of the organism. These lines, when colored, were always of the hue of the *moving* member. Now it is indeed strange that, where the two discrete positions were equal in intensity, brightness and extent, the hue of these secondary lines was always that of the object which moved. No report refers to the presence of intermediates like the unmoving member in quality. If these objects between are due merely to chance retinal irradiation of some sort why do they not occasionally assume the hue of the stationary member? As we regard the matter, the presence of these secondary lines indicates some functional peculiarity of the organism that may be of primary importance for the apprehension of visual movement. And it may be that the same functional conditions, which, at times, produce these linear extents serve likewise to create the vague, ill-defined and hazy colors which so frequently accompany the movement experience. Parts of a few reports are given below to show the general nature of these intermediate members.

> Two intermediate red oblique lines; over the rest of the field was a film of low chroma red (C, 150).
>
> A green horizontal line followed by a yellowish film beneath. Then I saw a greenish line a little lower down and then a red line came in below the position of the second green horizontal (Y, 75).
>
> A clearly defined intermediate position of a red oblique ⅓ of the way down (C, 150).

A green line moved rapidly to the bottom. I saw two successive positions intermediate to the beginning and final stages. These lines were fairly clean-cut and saturated (C, 75).

A green horizontal extent appeared in the visual field; then a moment later another green line somewhat like the first (chroma less) beneath it. Finally, a red line appeared a little lower down in a horizontal position (Y, 75).

Results; Series 17–32.—Here we used the stronger and the weaker lights. One light was given of an intensity sufficient to outline clearly the colored line upon the field. The fields were white, blackish, red, green and yellow. Upon the first two we drew red and green lines; upon the red we placed green lines; and upon the green and yellow, red lines. The stronger light was intense enough to wipe out the weaker. In serial exposition, the stronger light was turned on and off at periods of one second. The increase in brightness did not seem to disturb the observer.

Just as in the previous series we find here that the use of dissimilar hues brings out more reports of partial movement, although movement might be wholly optimal. The latter was especially true under serial exposition. Likewise the field of movement was partially or wholly covered with either red or green with a mixture of yellow. Again we find the presence of various intermediates of the hues of the moving object. Finally, we observe that the clearest kind of movement may be had even where the field is entirely that of the background. We cite the following reports to give some notion of the movement under these conditions.

A red oblique appeared upon a greenish-yellow background for a moment and then disappeared; and at the same instant a greenish line seemed to start just below and move down and come to rest as a horizontal line upon a brighter field of red. There was a distinctly *greenish* tinge over most of the sector although the red of the background was visible through the green overlay. The color of the moving line was spread over the ground much on the order of a varnish (C).

A green line appeared on a red field of medium chroma. Then the whole field suddenly brightened and I saw another green extent below. Then I saw an upper green line again in the first position. This happened five times. I saw a green line move from an upper position to a lower position, then up, then down, etc. Cloud-like greenish haze over part of the field (Y).

A red bar moving continuously up and down. The field is red over the upper and the lower ⅓. The middle ⅓ is exactly like the black background. I compared the two very carefully. Movement is over the whole field regardless of this gap. The field alternately grew lighter and darker (C).

A green line appeared in an oblique position. It disappeared by moving to a horizontal position. Movement was uniform and continuous although visually the background of red was observable over most of the field. Just above the final position of the line the field was greenish (C).

A bluish-red line at angle on yellowish field; then a more saturated and similar line on a high-chroma yellow field. I saw a bluish-red line move downward over part of the field (Y).

I saw absolutely continuous movement of a red line up and down with the impression that the red line was in some way detached from the green field. I got the red line outlined at various stages of movement between the end-positions. The field of movement is not the same as the background; it has a reddish tinge over it. I see the green *shine* through the reddish overlay (C).

A rather significant fact came out of these series (17–32) where, through the use of the dissimilarly colored members, O could discriminate more sharply between the two objects. Frequently it was not the first object which moved across the field to the position of the second. Rather it was the second object of a different hue which travelled away from the position occupied a moment earlier by the first. Wertheimer, as we know, claims that the temporal disjunction of the two objects provides for the completion of the neural process underlying visual movement; and Koffka represents in a simple diagram the way in which the cortical flow which is assumed to serve as the neural substratum of optimal movement spreads out. Let us assume a point in the cortex excited by the first object, followed a short time later by a second point corresponding to a second object. If the second cortical region is aroused when the neural excitation from the first has reached a point such that the two neural processes coincide near the second, the observer sees the first object move to the second. But here we get a weaker red or green object presented for a time; then a stronger green or red object is given. And under these conditions O may actually see the *second* assume its final position by moving over the field from the region of the *first*.

The use of the type of object employed in this series gave us an opportunity closely to compare our reports with those of Dimmick. Where we used the *whitish* background we had a total of 42 per cent. of reports of a grey lying somewhere between the lines or colored end-areas. But where we used

the black, red, green and yellow cards we did not obtain a *single* report of a grey such as Dimmick describes. This fact is all the more striking because we started the series with the white cards and O reported the greys. Then after we changed to the other cards O (Y particularly) remarked that "I no longer see the grey filling which I observed a short time ago." The intermediate grey appeared as a darkening of the field. When O was asked to describe it he reported that it was a "slight decrease in the brightness of the field, just as if the field had been slightly shaded." There is then a possibility, where a greyish field is used with colored stimulus objects, that O will see a grey somewhere between the lines; but where colored and black grounds are used the chance of such a grey occurring is negligible. This visual grey appears partly as a function of certain particular physical conditions without any outstanding significance for the movement experience in general. It is, as we now see, but one of various kinds of visual filling and cannot possibly be set up as of special importance.

Conclusion.—We find that the hue of visual movement perceived under the present conditions is absolutely dependent upon the hues of the stimulus objects. If red objects are employed the movement is always of *red* objects, although no color may be observed covering the total extent of the movement field. When green lines are used the movement is always of a green object. When unlike hues are employed the movement may be of one or both members but there is always an object-meaning. O can definitely say that it was not that object of that hue which moved but this object of this hue. Again we must agree with Wertheimer that movement may be perceived without any precise filling of a visual character being present at all stages to carry the movement. In this way we get the 'illusion' in its most striking form. But always, even across these "visual gaps," there is the meaning that it is an object of a certain hue which moves. Movement is not in any sense to be cut away from the hue of the object. It is here that we are unable to agree with Wertheimer. Movement as we see it is always possessed of

objective characteristics. From our study we can say furthermore that a mere change in hue is sufficient to change the character of the movement apprehended; objects of unlike hue giving fewer cases of optimal movement. This fact is of importance and must be seriously considered in any final explanation of visual movement. It may mean that a mere loss of identity between the two members interrupts the continuity of movement. To the three factors of intensity, distance and time, which, according to the *Gestalttheorie*, determine the nature of perceived movement, must be added that of quality, and while it is not so difficult to understand in terms of the doctrine the claim that the intensity of the stimulus, the spatial separation of the objects, and the temporal disjunction of the members, effect the total character of the movement seen, it is, according to the same way of thinking, extremely difficult to understand the particular rôle that quality plays in the whole matter. Finally, we can state with the highest degree of assurance that the existential correlate of visual movement under discrete retinal excitations with colored stimulus objects cannot be said to be a grey. That such a claim is not sustained by experiment is a sufficient warrant for its denial.

A MODIFICATION OF THE LIPPS-MEYER LAW[1]

BY PAUL R. FARNSWORTH

Stanford University

In a recent article[2] the writer summarized the work of Meyer on *tonic* endings in melodies, and added further facts on *atonic* endings which considerably extended the scope of the Lipps-Meyer Law (Law of the Number 2). In the present article, still more conclusive evidence will be given for the need of modifying this law.

Let us think of the tonal relationships in a scale where c has been made the tonic. This is done by playing the scale of c a number of times until the tone *c* is felt as *the important tone* of the scale. The scale ratios are then usually understood as

c	d	e	f	g	a	b	c
16	18	20	21	24	27	30	32

It can be readily seen that the extraction of the powers of 2 does not alter the existing relations. The following series of ratio symbols then appears

c	d	e	f	g	a	b	c
2	9	5	21	3	27	15	2

B♭ in this system would be 7. The tone which bears the ratio-symbol 2, the tonic, has the property of being preferred over all other tones as the melody ending. This is the substance of the Lipps-Meyer Law. If g is made the tonic, the ratio-symbols shift so that g is ratio-symbol 2, d is 3, a is 9, *etc.*

In the previously mentioned article, the writer found that when the tonic is absent, the ratio 3 is preferred most extensively as the point of ending for a melody. The ratio-symbol

[1] This paper was read in part at the Berkeley meeting of the Western Psychological Association, August, 1925. The experiments were carried out in the Psychological Laboratory of Ohio State University.

[2] P. R. Farnsworth, *Amer. J. Psychol.*, 1925, *36*, 394-400.

5 is next most preferred and 7 least in the proportions 41, 34, and 25 per cent. respectively. This was ascertained by comparing the sequences b♭ g e (7, 3, 5, where c is 2, and e is the lowest note), g e b♭ (3, 5, 7) and e b♭ g (5, 7, 3) by the method of paired comparisons. In a second article, the data showed that specific training on any ratio-symbol will raise its preference value as a melodic ending.[3]

According to Meyer [4] and Bingham,[5] the lowest tone of a combination will be preferred as a point of melodic ending (other factors being equal). Although this factor of 'falling inflection,' as it is called, was partially taken care of in the previous work, a possibility existed that it still might somewhat effect the results. A technique was therefore developed in the present undertaking whereby this effect was eliminated. It was also desired to see the relation between number of subjects and tonic effect. The data on falling inflection incidentally corroborated those of Meyer and Bingham.

Method

In this series all successions had turns at high, middle and low position. This was accomplished by giving the tonal sequences on three successive days with three different tonics (ratio-symbol 2).

Musical Notation	Ratio-Symbol	Vib. Ratio	Vib. Rate (d.v.)
First Day—C is Ratio-Symbol 2			
g	3	6	96.89
g′	3	12	193.77
e′	5	10	162.94
b♭	7	7	115.22
b♭′	7	14	230.43

[3] P. R. Farnsworth, *Amer. J. Psychol.*, 1926, *37*, 116–122.
[4] M. F. Meyer, *Amer. J. Psychol.*, 1903, *14*, 456–478.
[5] W. V. Bingham, *Psychol. Rev. Monog.*, 1910, *12* (no. 50).

Second Day—G is Ratio-Symbol 2

b	5	10	122.07
b′	5	20	244.14
f	7	7	86.31
f′	7	14	172.63
d′	3	12	145.16

Third Day—E is Ratio-Symbol 2

b	3	6	122.07
b′	3	12	244.14
g♯	5	5	102.65
g♯′	5	10	205.29
d′	7	7	145.16

As will be noted from the vibration-ratios and -rates, ratio-symbol 3 was the lowest for the first day, 7 the second day, and 5 the last. A fairly similar range existed from day to day.

A reed-organ was employed which had a tempered scale, so the ratios were not exact. But according to Meyer,[6] exact ratios are not necessary. "It is natural that there should exist a limit to the possibility of relation between that which can be understood and that which can be offered to the hearer. If the question be to understand one of two sounds which form an interval within a melody, the reasonable limit of the deviation of the sound is located in the middle point of that interval; at that point the interpretation is arbitrary in a way that both sounds can be understood with equal facility by anyone who listens. In practice, if the mistuned sound does not deviate more than one-third of the theoretical sound; that is to say, if it remains farther than two-thirds from the other sound, the ear of him who hears or interprets the music does not admit of any equivocation. It is to be inferred from the above statement that the practical limit of the deviation depends upon the size of the intervals used."

The subjects were two groups of students in esthetics; one of 40 and the other of 22. In each set of experiments the subjects were divided into (*a*) trained subjects having one year or more of musical training, and (*b*) untrained subjects. The directions were as follows:

"You will hear two series of three tones each which you are to compare. Which series sounds more finished, more complete, more at rest? If it is the first, mark 'one' on your paper; if it is the second, mark 'two.'"

[6] M. F. Meyer, *La Antorcha* (Mexico), 1925, *1*, Núm. 16. The article is written in Spanish.

A random order was followed, and the sequences were paired five times, making a total of thirty judgments. Before the test-series was given each day the experimenter played the scale of whatever tone was tonic. The sequences were played with a falling inflection.

By this method it was possible to work out, not only the preferences for the ratio-symbols 3, 5, and 7, but also for the three positions, low, middle, and high.

PREFERENCES FOR RATIO-SYMBOLS

Class of 40

	3	5	7
	57 63	43 —	
		58	37 42
Tr.	42	35	23
Unt.	38	33	29
Ave.	40	34	26

Conclusion: Ratio-symbol 3 is preferred over 5 in the proportion 57 : 43, and over 7 in the proportion 63 : 37; ratio-symbol 5 is preferred over 7 in the proportion 58 : 42. Expressed together, ratio-symbols 3, 5 and 7 are preferred in the proportions 40, 34, and 26.

Class of 22

	3	5	7
	52 56	48 —	
		50	44 50
Tr.	37	32	31
Unt.	35	34	31
Ave.	36	33	31

Conclusion: Ratio-symbol 3 is preferred over 5 in the proportion 52 : 48, and over 7 in the proportion 56 : 44; ratio-symbols 5 and 7 are equally preferred. Expressed together, ratio-symbols 3, 5, and 7 are preferred in the proportions 36, 33, and 31.

PREFERENCES FOR POSITION IN FALLING INFLECTION

Class of 40

	Trained	Untrained	Average
High	29	26	28
Middle	35	33	34
Low	36	41	38

Conclusion: The high, middle and low sequences are preferred as endings in the proportions 28, 34, and 38.

Class of 22

	Trained	Untrained	Average
High	29	27	28
Middle	31	31	31
Low	40	42	41

Conclusion: The high ,middle and low sequences are preferred as endings in the proportions 28, 31, and 41.

SUMMARY

A. Preferences for Ratio-Symbols 3, 5, and 7.

(1) A group of 40 subjects gave results very similar to the combined results given in the writer's previous article.

(2) A group of 22 subjects displayed variations in the same directions but of smaller range than those of the larger groups. This shows that a large group of subjects is necessary to bring out the effect.

(3) The trained subjects showed a greater range of variation than did the untrained.

(4) These results would lead to a restatement of the Lipps-Meyer Law to read somewhat as follows: "The ratio-symbols 2, 3, 5, and 7, when employed as endings, display repose effects in the inverse order of their size. Specific training can markedly change this order." It should be noted that nothing definite is known of the conditions responsible for this effect, or of the limits of the tonal range in which it exists.

B. Falling Inflection.

(1) Within the range used both groups gave marked responses to falling inflection.

(2) This effect was relatively independent of the size of the group.

(3) The variation was more marked with the untrained group, which apparently reacted less to the ratio-symbols. Hence the falling inflection had no complications from opposing forces.

(4) These results corroborate those of Meyer and Bingham.

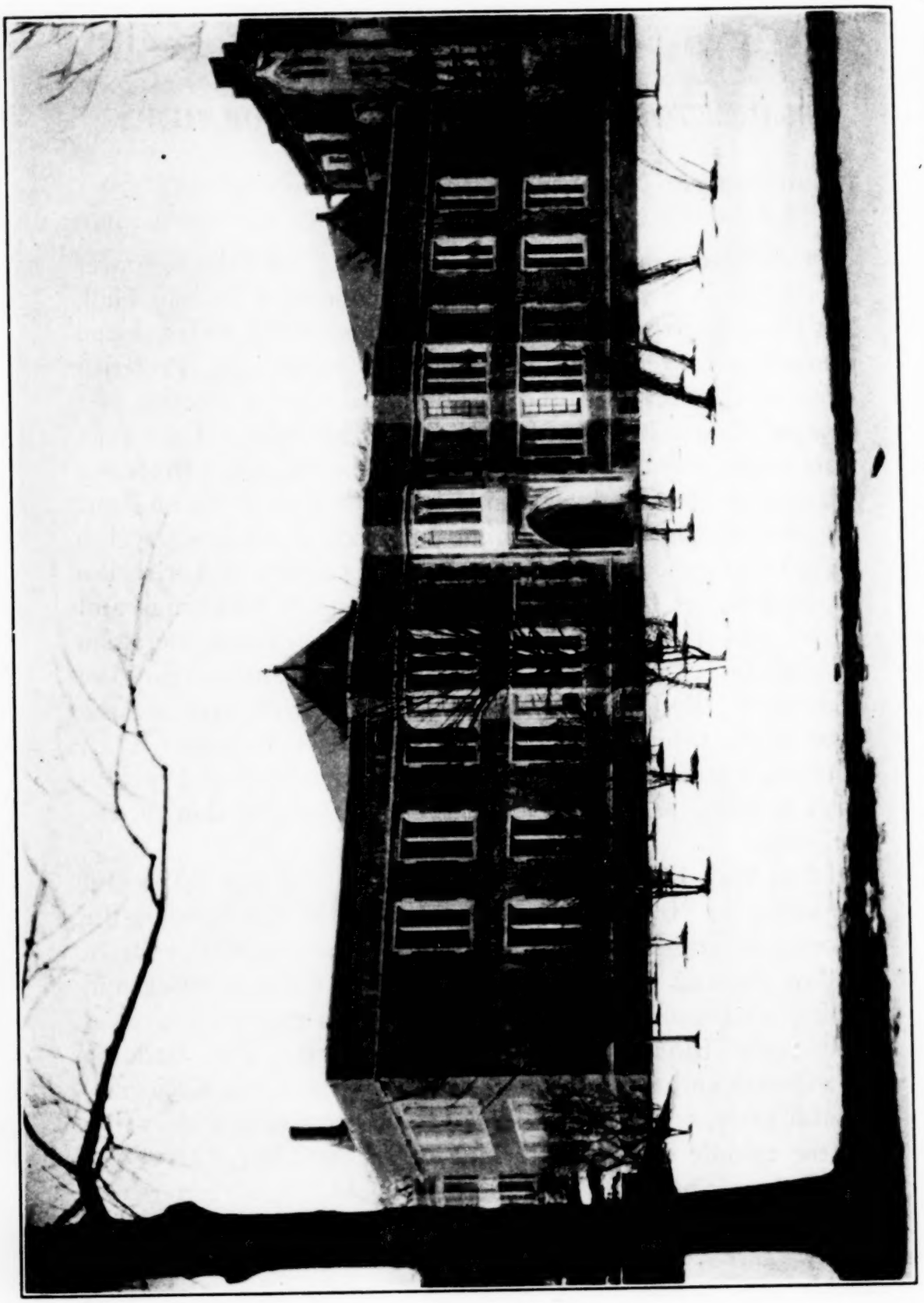
PRINCETON PSYCHOLOGICAL LABORATORY

BY H. S. LANGFELD

Princeton University

Until last year research in psychology at Princeton was conducted in a few rooms on the top floor of Nassau Hall, but these quarters were very inadequate both as to space and general arrangement. Through the efforts of Professor Howard C. Warren a fund was obtained for the erection of a new building to be called Eno Hall after Mr. Henry Lane Eno, who contributed the larger part of the money. Professor Warren spent several years in visiting laboratories throughout the country and in ascertaining the needs of modern psychological research, and on the basis of this study, and with the coöperation of Professors Brigham, Dunlap, McComas and Zigler, he devised the plans of the building. The cornerstone was laid in January 1924 and the Department moved into the laboratory the following December. In April 1925, at the time of the meeting of the Experimentalists, Professor E. B. Titchener gave an address entitled 'Experimental Psychology: A Retrospect' in honor of the formal dedication of the building.

Eno Hall is 25 ft x 128 ft and consists of two floors and a basement. It is of fireproof construction throughout, the floors and stairs being of reinforced concrete, the walls of hollow tile and concrete with an outside finish of brick and white sandstone trimmings. The roof is flat with a solid balustrade running completely around it. The building faces north and south. There are two entrances in Collegiate Gothic style, one in the middle of the north side and the other in the middle of the south side of the building. Over the north door is inscribed 'Psychology' in old English letters and over the south door the motto from the Delphic Temple, 'Γνῶθι Σαυτόν.' Each entrance has a small vestibule with inside and outside doors.



PRINCETON PSYCHOLOGICAL LABORATORY.

Research Rooms.—The principle followed in the arrangement of the research rooms was to have a large number of small rooms which could be used either for individual research, for undergraduate training courses, or for offices. These rooms are therefore all approximately of the same size and have the same arrangement for electric current, gas, etc. They are on the first and second floors and on both sides of the corridors, which run almost the length of the building and are slightly off center in order to allow for larger rooms on the south side. There are twenty of these rooms, the ones on the south side being 9 ft x 14 ft. Six of the rooms have bay windows which give them several additional feet. All of the rooms have doors with ground-glass panels leading into the corridors and four of the rooms have a second solid door to keep out light and sound. There is also a door between adjacent rooms, so that they can be arranged in suites if desired. The longest row of communicating rooms is on the south side of the second floor and consists of six rooms. On the first floor four rooms can be thrown together and these rooms are generally used for undergraduate instruction and research. There are also suites of three rooms. In each room there are three outlets for direct current on each side wall, and one outlet for alternating current, and also three 'dead' sockets on each side wall for communication between rooms. In addition there are in each room outlets for gas and compressed air, and two wooden instrument rails around three sides of the room to which apparatus can be attached. The rails are either bolted to a similar rail on the opposite side of the wall or screwed into expansion plugs. The lower rail is 3 in wide and 40 in from the floor. The upper rail is 4 in wide and 3 in below the ceiling. The latter rail can also be used as a picture molding. Ten of the rooms are provided with a work bench, which is built over the radiator beneath the windows and extends the width of the room.

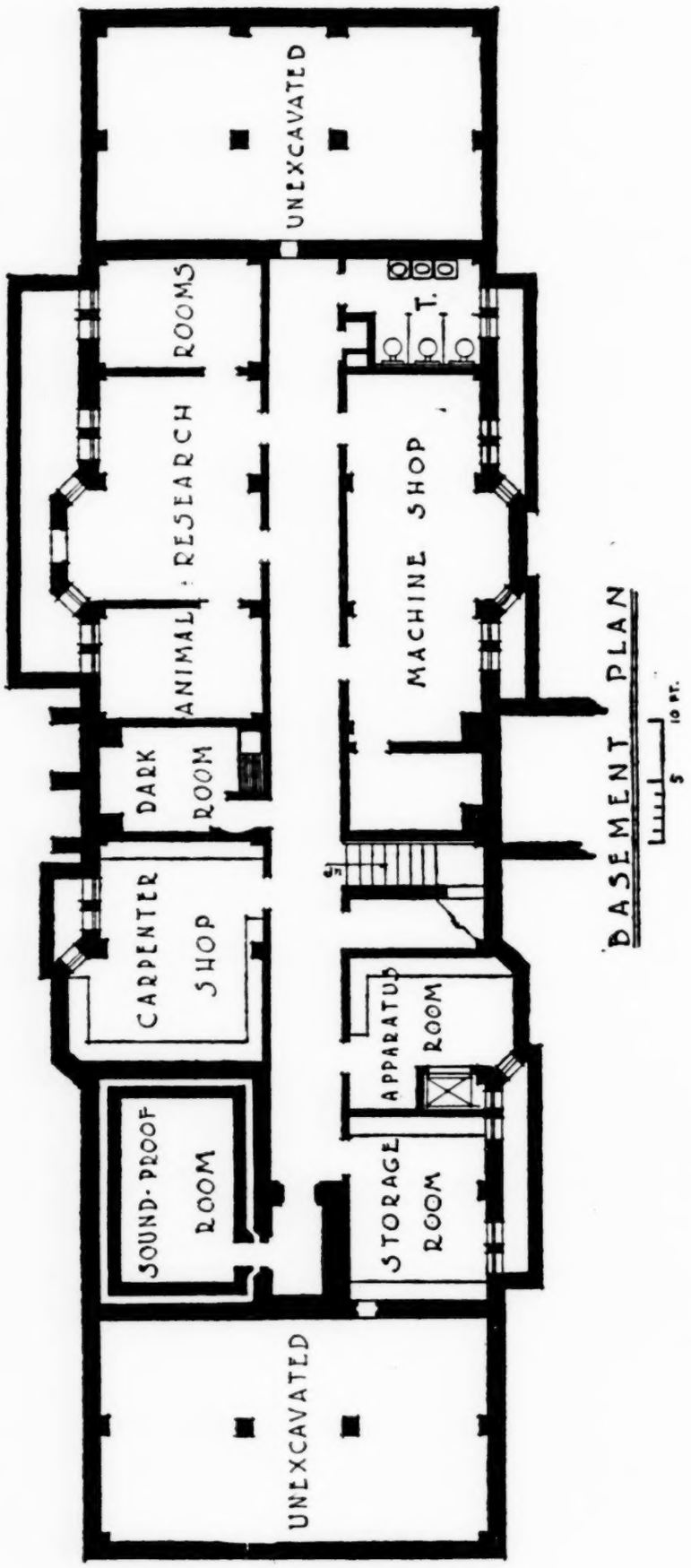
Dark Room.—There is a dark room on each floor. All are provided with outlets for electricity, gas and air, and instrument rails, as the other research rooms are. They have also a soapstone (Alberene) sink and hot and cold water.

PRINCETON PSYCHOLOGICAL LABORATORY

LIBRARY
T.
T.
Vestibule
LECTURE RO.
40 Seats
UNDERGRADUATE
RESEARCH LAB.
DARK
ROOM
APPARATUS
ROOM
PSYCHOLOGY · BUILDING
PRINCETON · UNIVERSITY
PRINCETON, N.J.
1924
N
FIRST FLOOR
Scale in Ft.

The dark room which is in the basement is 9 ft x 14 ft. It has no windows. Ventilation in this room is provided by a light-proof ventilator from the next room. The entrance is made light-proof by means of two doors which can be fastened together by a brass bar to form a sector of 45°, and which swing like revolving doors except that they only revolve through a semicircle and then swing back, instead of making a complete revolution. Rubber strips are attached to the edge of the doors to cover the cracks. When the doors are fastened together by the bar, one of them is always in contact with the curved wall of the vestibule, so that light can not enter at any point in the swing. The walls, ceiling, and floor of this room are painted black. The dark room on the second floor is 12 ft square. It also is painted black and has a similar arrangement of doors. It has two windows provided with light-proof shutters. The dark room on the first floor is also 12 ft square and its walls and ceiling are painted white. It likewise has two windows provided with light-proof shutters. The entrance has double doors one behind the other. This room is intended primarily for undergraduate work.

Sound-proof Room.—This room is in the basement and access to it is through a smaller anteroom 7 ft 8½ in x 4 ft 4½ in. This latter room has two doors of 2 in thickness at the entrance with a passage 1 ft deep between them. The inner door is heavily padded round the edges. The walls of this room consist of 1-in Celotex and plaster, 8½ inches of hollow tile, and 13½ inches of concrete. The floor, which is covered with ¼-in cork, consists of 1-in cement finished on 3-in concrete slab, and beneath this slab is a bed of cinders. The walls and ceiling are painted black. The entrance to the S. P. room proper is through a vestibule 2½ ft in depth. There is a 2-in thick wooden door padded round the edges at each end of the vestibule. The S. P. room is 15 ft 4 in x 9 ft 7 in and is 8 ft 5 in high. It is virtually a building within a building. The walls consist of 1 inch of Celotex and plaster, 4 inches of hollow tile, and 4 inches of air space which separates the room from the concrete and hollow tile construction of the

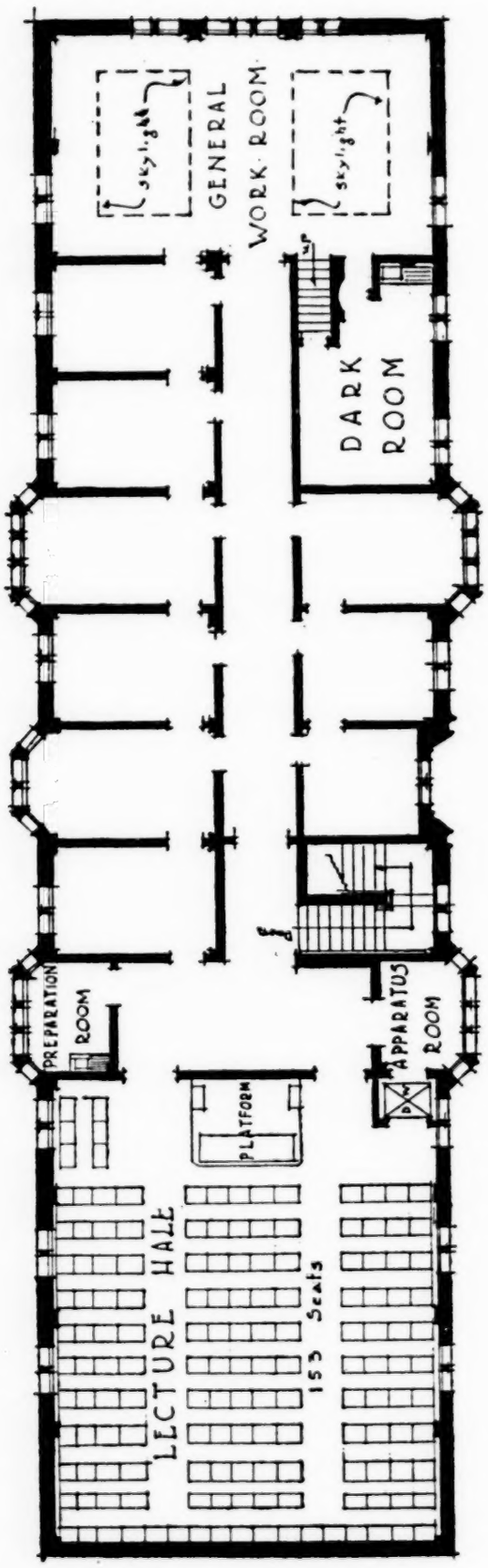

UNEXCAVATED
ANIMAL RESEARCH ROOMS
T.
MACHINE SHOP
DARK ROOM
CARPENTER SHOP
SOUND-PROOF ROOM
APPARATUS ROOM
STORAGE ROOM
UNEXCAVATED
BASEMENT PLAN
5 10 FT.

main building. The ceiling is of similar construction to the walls, and above the air space of the ceiling is a pre-cast slab of concrete, which is supported by the walls of the main building. The floor consists of ¼-in cork over 7 inches of concrete. Beneath the concrete is a 7¾-in bed of sand and then another 6-in concrete slab and finally a second bed of sand 8 inches deep. The walls and ceiling are painted black. The room is supplied with electricity, gas and air, and two apparatus rails, like the other research rooms. The pipes have a flexible rubber joint between the rooms, and both pipes and wires can be detached from the main system if absolute isolation is desired.

Animal Research Rooms.—Three rooms in the basement have been arranged for animal experimentation. The middle room is 19 ft x 14 ft and has two doors into the corridor. From this room there is access to two smaller rooms 14 ft x 9 ft 4 in. Each room is equipped with gas, electricity, compressed air, water, and two appratus rails. There is a drain in the middle of each floor to facilitate thorough cleaning. A door in the middle room gives access into a small area-way with an overhead grill where the animals can be in the open air.

Physiological Psychology Room.—On the first floor a room 19 ft x 11 ft 9 in has been arranged especially for dissecting work. A soapstone table 2 ft wide and 19 ft long runs the length of the room just below the window. Within easy reach of the students when they work at the table are several outlets for gas and compressed air. At one end of the table is a soapstone sink with hot and cold water. The room can also be used for courses in experimental psychology.

General Work Room.—On the second floor there is a large room 19 ft x 32 ft, which is used for class experiments, color research, photographing, drafting, etc. It has two large skylights in the ceiling, which together with the eleven windows give an excellent illumination. The sky-lights can be opened for ventilation purposes. The room is equipped with apparatus rails and outlets for gas, electricity, and compressed air, and there are several large cases for instruments. The stairs lead from this room to the roof, which is furnished



SECOND FLOOR PLAN.

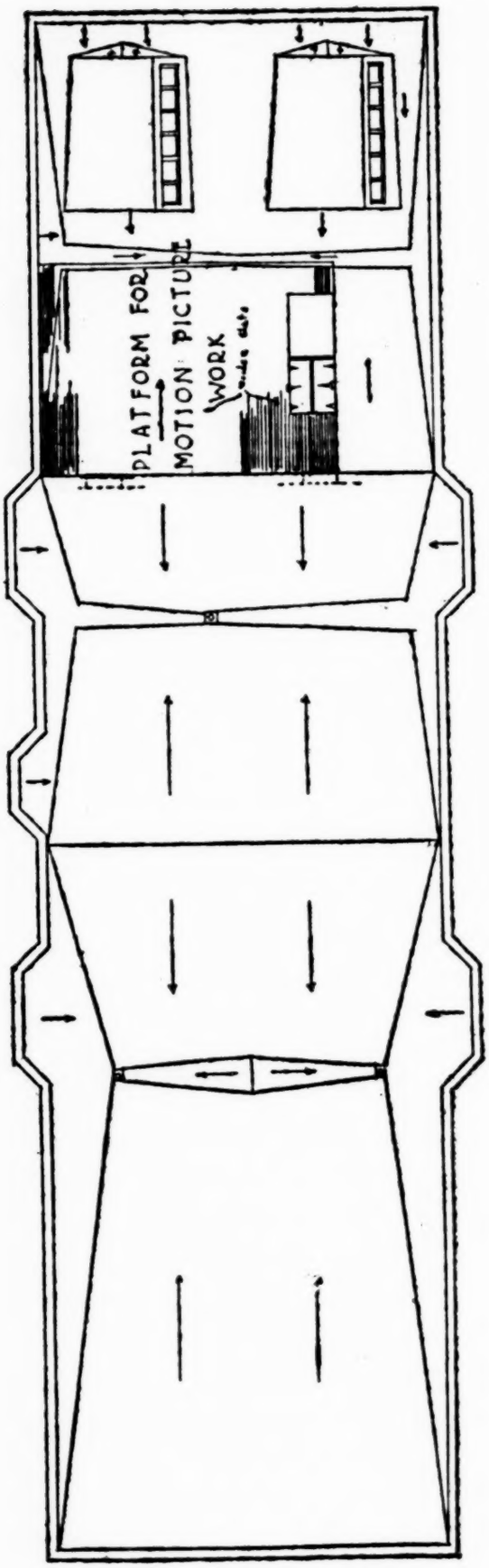
with a broad wooden platform for photography. Near the platform is an electric outlet for a motor or lamp.

Library.—The combined library and seminary room is on the first floor directly under the work-room and is of the same size as the latter. Steel stacks from the Library Bureau, with shelves for 6,000 volumes, occupy half the room. On the other half of the room there are one long and two smaller tables for reading and seminary work. The magazine rack which holds about forty journals occupies one wall.

Apparatus Rooms.—There are three rooms constructed especially for apparatus. The one in the basement is approximately 12 ft x 13 ft. Like all the rooms in the basement except the dark room and sound-proof room, it receives light from windows opening into an areaway. It has cases running along one side wall and several shelves against the other wall. The room on the first floor is 12 ft square and has specially built cases with sliding doors which line two of the walls. The second floor room, which is next to the large lecture room, is 6 ft x 9 ft exclusive of the bay window. There is one case similar to those on the first floor. By means of a hand elevator instruments may be transferred from one apparatus room to the other and to the large lecture room.

Preparation Room.—On the second floor on the opposite side of the corridor from the instrument room is a room 6 ft x 9 ft which is meant for a preparation room in conjunction with the lecture room. It has a soapstone sink with hot and cold water. It can also be used for research purposes if so desired.

Lecture Rooms.—There are two lecture rooms. The larger one, 19 ft x 32 ft 9 in, is on the second floor at the other end of the building from the work room. It contains seats for 150 students. The floor rises toward the rear so that the heads of the students on the last row are on a level with the head of the lecturer. The smaller room which is on the first floor next to the physiological psychology room is 12 ft 4 in x 19 ft and has seats for 40 students. Lecture desks in both rooms are supplied with gas, compressed air, and direct and alternating current. The desk in the smaller room has an



ROOF PLAN
5 10 FT.

acid-proof top. Each room also has a Thompson combined projection lantern and projectoscope.

Machine Shop.—This room is 11 ft 7 in x 31 ft 4 in. It has six windows and two doors with glass panels and is thus well lighted. There is an island floor 7 ft x 29 ft which is completely separated from the rest of the building in order that the vibrations from machines placed upon it may not be communicated to the other rooms. On this floor are placed an 11 in x 5 ft Monarch Jr. Engine Lathe (Monarch Machine Tool Co., Sydney, Ohio); No. ½ Van Norman Duplex Milling Machine (Van Norman Machine Tool Co., Springfield, Mass.); a P–4 Sensitive Drilling Machine (Charles G. Allen Co., Barre, Mass.); and a Ball Bearing Motor Grinder (J. G. Blount Co., Everett, Mass.). All of these machines as well as the Stark Precision Bench Lathe and Drilling Machine (Stark Co., Waltham, Mass.), which is off the island floor in the bay window, have their own motor, either attached to the machine, or on the floor. There are no overhead or wallshafts to cause vibrations. A Machinist's Bench (New Britain Machine Co., New Britain, Conn.) fits in the space in front of two of the windows. Near it is a No. 3 Greenerd Arbor Press (Edwin E. Bartlett, Nashua, N. H.). The motor and blower for the compressed air system are on the island floor and the air tank is fastened on the wall about 5 ft from the floor.

Carpenter Shop.—The room is 13 ft 6 in x 18 ft 4 in excluding the bay window. The Bench Circular Saw with self-contained motor (J. D. Walker and Co., Chicago, Ill.) is in the center of the room. In the bay window is a wood lathe (American Woodworking Machine Co., Montgomery, Pa.). The belt runs from the motor on the floor to shafting on the wall and thence to the lathe. (A machine from the old laboratory. As this machine is used only occasionally and then only for a short period of time, it was thought that the vibration would not interfere seriously with the experiments. A modern lathe with self-contained motor would be preferable.) There are two work benches, a tool board, shelves and lumber racks. The students are allowed to work in this room, but not in the machine shop.

Storeroom.—A room 11 ft 7 in x 15 ft 3 in has been furnished with shelving around the walls for the storage of records, old journals, etc.

Electrical Supply.—The alternating current, which goes to all the rooms, is obtained from the general circuit of the University. Direct current is obtained from two sources, storage batteries and a 110-volt generator. There are 35 Edison B4 batteries arranged in six units of five cells, one unit of three cells, and one unit of two cells. Each cell gives approximately 1.2 volts. These cells are placed in a cabinet in the carpenter shop. The generator is at one end of the machine shop near a large switch-board, which was made to order by the General Electric Co. Wires lead from the batteries and generator to the board and from the board to almost every room in the building. There are six lines to each room, but where there are adjoining research rooms, each line serves two rooms; that is to say, one can plug into the same line from either side of the wall. The switchboard is so arranged that one can connect the current from the generator or from one or more units of batteries with any room. Seven rooms can take off current from the generator at the same time. There is also a rheostat on the switchboard by which the voltage may be increased or cut down when desired.